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M E D I C A L C O M M E N T A R I E S.

P A R T I.

CONTAINING
A PLAIN AND DIRECT ANSWER TO
PROFESSOR MONRO jun.

INTERSPERSED WITH
REMARKS on the STRUCTURE,
FUNCTIONS, and DISEASES
O F
SEVERAL PARTS of the HUMAN BODY.

By WILLIAM HUNTER M. D. K

The SECOND EDITION.

L O N D O N,

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A D V E R T I S E M E N T,

Prefixed to the first Edition, viz. An. 1762.

THE Author of these Sheets has formed a design of offering to the Public, from time to time, his Observations in Anatomy, Surgery, and Midwifery; and has therefore given such a Title to this First Part, as may in some measure comprehend the whole of his scheme.

The Second Part will contain an account of the Gravid Uterus; the publication of which has been so long retarded only by the author's desire of sending it into the world with fewer imperfections. In the long interval of time, since proposals for executing this work were first given out, some favourable opportunities of making observations on this subject have occurred, and, it is hoped, have furnished such matter of improvement, as may make some compensation for the delay.—The plates, illustrating the description of the Gravid Uterus, most of which have been engraved many years, will be published separately in a large folio volume.

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SUPPLEMENT.

T H E

T H E I N T R O D U C T I O N.

A Controversy relating to any improvement or discovery in the arts, when there are complaints of unfair dealing, is an awkward subject for a party principally concerned. The writer must plead his own cause; and where he thinks himself in the right, and ill-treated, he will be led to speak in his own favour, and to the prejudice of another person; both which a man of an ingenuous turn of mind would willingly avoid. Another disadvantage, under which a writer in such a dispute must appear, is, that he will often seem to be magnifying trifles, claiming what is not worth his possession, and proving or denying things of no consequence; for the subject in dispute is often of trivial moment in itself, but becomes important to the parties concerned, when their moral characters are affected by it. Hence the reader is naturally first offended with an importunate eagerness in the writer, then tired of so much about nothing, and, perhaps, condemns both, without attending to what either has to say.

Another circumstance, as unpleasant as either of those already mentioned, is, that in order to do justice to the cause, it may be necessary to say many things, which an author would wish to avoid; he may be obliged either to suppress his evidence, or to call upon his friends, and publish the matter of private conversation; in short, to mention many things, which though, for some reason or other, it may be disagreeable to relate them, are yet necessary in the defence of truth.

As these are the genuine sentiments of the author of the following pages, it is natural to believe that he would willingly have declined a paper-war with Dr. Alexander Monro jun. of Edinburgh. But the treatment, which he has received from that gentleman, has been so very singular, that it was insisted on by his friends, that he should publish a full state of the case, supported by unquestionable testimony. He urged, that his original complaint against Dr. Monro was not worth the attention of the public,
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in any other way than as a paragraph in a literary journal ; and that to answer the illiberal censures, and expose the subterfuges, in the Doctor's pamphlet, he should be obliged to descend to things of too trivial a nature for the notice of the public. They observed, on the other hand, that this step was *become necessary* ; that the necessity of it would prove its excuse, &c. They prevailed.

If any part be found to be severe, it will be the facts rather than the language. Justice calls for facts : the intelligent reader will make his own reflections, and will give his censure or his approbation, according to his opinion of the merits. The dispute lies within a small compass ; and the little, that has been already advanced on this side of the question *, is concise and to the point. Dr. Monro's pamphlet † is prolix, vague, and without precision. These are faults which may happen in controversial writing, where the cause is good and the heart upright ; but we more commonly find them where it is necessary to stifle truth, to disguise falsehood, or to throw obscurity over the whole, that the fair inquirer may be tired, and forced to declare, that the question is too difficult and perplexed for him to decide.

The acrimony so remarkable in Dr. Monro's pamphlet may be excused by those who entertain a favourable opinion of its author. They will tell us, that anger is a very natural passion, and is often an attendant upon an honest heart. We grant it ; but still it is a fault, inasmuch as it gives a suspicion of a weak or of a bad cause, and of a mind prone to do an injury ; and experience often verifies the old observation, that the aggressor is the most irreconcilable enemy ‡.

But

* Critical Review for Nov. 1757, Art. IX. and for Dec. 1757, Art. IX. See Appendix, No. III. & V.

† Observations Anatomical and Physiological, &c. Edinburgh, 1758, 8vo.

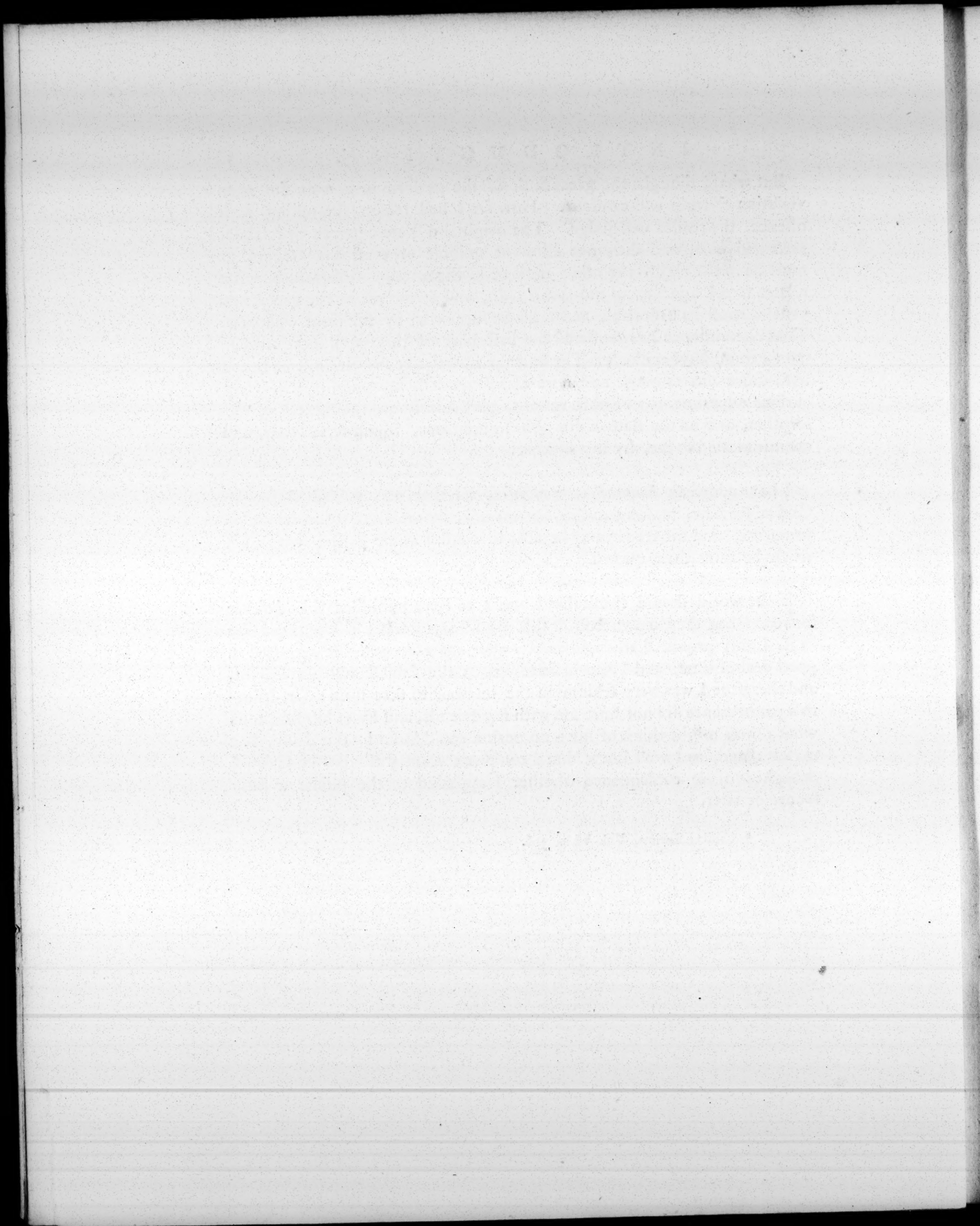
‡ If the reader should have forgotten the acrimony of Dr. Monro's performance, and think these hints too strong, we would beg the favour of him to say, whether *gentlemen* treat any living character, above the rank of the pillory, in the following manner. In his second page, he thinks it *highly probable* that Dr. Hunter's *conscience must have rejected what his pen affirmed*. In page 4, he *conjectures* that Dr. H.'s intention was *to catch at any occasion to propagate what was false and injurious, being conscious that it was so*. P. 10, he represents Dr. H. as *drove by the rage of detraction to the most frivolous resources*. P. 11, he says, that Dr. H.'s proceedings *must, to every man of common sense, appear not only highly unjust and malevolent, but equally weak and ridiculous* : and he mentions something as *the last effort which Dr. H.'s imagination has been able to suggest for throwing a reproach upon him*.

But what I thought the great fault of the performance, was the open violation of truth and candour. How far I had reason for taking such offence, the reader will judge. The original dispute between us is about *facts* and *dates*, and does not allow of quibble or evasion. One of us must be in the right, the other must be in the wrong. Therefore, what relates to the injection of the *tubuli testis*, and to the use of the lymphatic vessels, must be historical, and must be supported by sufficient vouchers. That circumstance has rendered the first part of this work more tedious than I could have wished, The latter part, I flatter myself, will be found both more entertaining, and more useful, as it is interspersed with many curious experiments and observations, particularly on the subject of absorption, and on the state of the *testis* in the *fœtus*, which were made, and communicated to me, by my brother.

I have added an Appendix, containing all that was published in the Critical Review, relating to this controversy, before Dr. Monro wrote his pamphlet, that the reader may be able to consult those papers with ease, when he finds them quoted.

If the world should think that I ought to have published this defence sooner, I beg they may recollect that when my friends first engaged me in it, I only promised to do it *with proper opportunity* *. It required a good deal of time, and I had little to spare: the subject was unpleasant, and therefore I was very seldom in the humour to take it up: Dr. Monro's performance did not hurt me with my friends, and I was less solicitous what others might think of me: no person could suffer by the delay, but myself: and, as I well knew that I could make every thing very clear, I thought it of no consequence whether I appealed to the public a little sooner or later,

* Critical Review, Vol. VI. p. 316.



C H A P. I.

Of the Injections of the *Testis*.

IN the year 1757, I informed the public * what I had done upon this subject, in the following words.

“ About the beginning of November, 1752, in presence of Mr. Galhie, and some others, I injected the *Vas deferens* in the human body with mercury, and by that method filled the whole *Epididymis*, and the tubes that come out from the body of the *Testis* to form it: and observed, in this operation, that the mercury continued to run, and the body of the *Testis* to become gradually more turgid and heavy, for some time after the external parts were compleatly filled. I shewed this preparation next night at my public lecture, said that I believed we should find the internal *tubuli* likewise filled, but that I would not venture to open it, till I had got another, lest I should spoil what was already a valuable preparation; and desired my brother to lose no opportunity of making the trial.

“ This was communicated as a piece of anatomical news to Dr. Donald Monro, then at Edinburgh, by a letter from Dr. Garrow, physician at Barnet, some time in the same month.

“ In some such time as a week or fortnight, after the first public demonstration, my brother made the trial, and succeeded. He shewed me the *Testis* opened, and the tubular internal substance very generally filled with mercury. This preparation, which I still preserve, I shewed at my public lecture, that very evening, with marks of being pleased with the discovery. In my next course of lectures, viz. Feb. &c. 1753, and in every course since that time, I have shewn the same, and some other preparations of the same kind; and always gave the history of the discovery, to avoid taking that share of it from my brother which belonged to him.”

Whether this was a fair relation of facts, though unpleasant to Dr. Alexander Monro jun. or whether it was false, and consequently most impudent and weak, because it could have been disproved by an hundred living witnesses, the reader will determine, when he considers the fol-

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lowing

* Critical Review for Nov. 1757, Art. IX. See Appendix, No. III.

lowing testimonies *. That he may better understand them, I beg leave to observe, that I have always given two courses of lectures every winter, each course lasting about three months. The first, and what is called the Autumn course, is finished before New-years-day; and the second, called the Spring course, begins about the 20th of January.

Mr. Galhie, of Spital-square, surgeon, the gentleman abovementioned, in the account he gives me under his hand, says;

“ Having made no memorandum of the *Testis*, in which your brother
 “ had filled the internal tubes with mercury, I cannot be positive as to
 “ the precise time it appeared at lectures; I remember, indeed, that the
 “ preparation was made *within* a few weeks after you had injected the
 “ first *Testis*; and, as it was shewn to the pupils the same evening it
 “ was finished, it *must without doubt* have been publicly demonstrated
 “ sometime in the autumn course, 1752.

Spital-square,
 Nov. 24, 1758.

R. GALHIE.”

Mr. Watson, of Marlborough-street, surgeon and reader of anatomy †, has given the following account of the matter.

“ In the beginning of the Autumn course of the year 1752, Dr.
 “ Hunter got a *Testis*, filled it, and shewed it at public lecture. In
 “ this preparation, the whole *Epididymis*, and the ducts between it
 “ and the body of the *Testis* were all distended with mercury; and, al-
 “ though the mercury had passed very probably into the body of the
 “ *Testis*, yet the Dr. did not care to run any hazard of spoiling so fine
 “ a preparation to demonstrate this circumstance. I cannot charge my
 “ memory with the exact time; but I very well recollect, that it was
 “ *but a few days* after when I saw the whole *Epididymis*, the communi-
 “ cating *Tubuli*, and the *Tubuli* in the body of the *Testis*, finely filled
 “ with mercury. I remember perfectly well, that I first saw this second
 “ preparation

* Let me once for all do justice to the character of every gentleman, who has done me the favour of giving his evidence in this dispute, by declaring, that none of them were officious witnesses; and that none of them meant to take part with either side, but to say what they knew to be truth, when application was made to them for that purpose.

† I take the opportunity with pleasure of doing this gentleman the same justice, that I did him at my lectures with regard to his observations upon the *Testis*, by declaring that he first shewed me the ducts coming out from the *Testis* to form the *Epididymis*, in a preparation where he had traced them by dissection with great accuracy.

“ preparation in the Dr’s study, one evening after lecture. Both these
 “ preparations were produced and demonstrated by the Dr. in the
 “ Autumn course of the year 1752; the one *within a few days* of the
 “ other. In this relation of facts, I have strictly adhered to truth; nor
 “ have I declared more than I was an eye-witness to.

Great-Marbro’ Street,
 Oct. 26, 1758.

HENRY WATSON.”

Mr. Davenport, of Norfolk-street, surgeon, says;

“ I declare that I attended both the first and second course of lectures,
 “ of the winter, 1752-3, with Dr. Hunter, and remember distinctly,
 “ that it was *in one and the same course*, that Dr. Hunter first shewed
 “ the *Epididymis*, and the internal *Tubuli Testis*, filled with quicksilver*.
 “ To the best of my remembrance, and from consulting my notes, it
 “ was in the course of October, November, and December, 1752.

London,
 Oct. 24, 1758.

R. DAVENPORT.

Mr. Davies, of King-street, surgeon, gives me the following attestation.

“ I attended Dr. Hunter’s course of lectures of October, November,
 “ and December, 1752, and can declare with the strictest truth, that,
 “ *in that very course* he shewed his pupils first the *Epididymis*, and
 “ then the internal substance of the *Testis*, filled with quicksilver.

King-Street, Covent-Garden,
 Oct. 26, 1758.

GARLAND DAVIES.”

Mr. Pile, of Parliament-street, Westminster, writes as follows;

“ I attended Dr. Hunter’s course of lectures of October, November,
 “ and December, 1752, and remember distinctly, that *in that time* he
 “ shewed publicly first the *Epididymis* filled compleatly with quick-
 “ silver, and then the internal *Tubuli* of the *Testis* likewise filled with
 “ quicksilver.

Parliament-street,
 Oct. 25, 1758.

DOM. PILE.”

Mr.

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* This is as much to our purpose, as if he had remembered the very month and day, because Dr. Garrow’s letter (admitted by Dr. Monro) proves that the first preparation was shewn in the Autumn course, 1752.

Mr. Nicolay, of Green-street, Leicester-square, surgeon, gives me the following testimony.

“ I attended Dr. Hunter’s Autumn course of lectures of October, November, and December, 1752, and several other courses afterwards, and do declare, that I was present when he first shewed the *Epididymis* filled with quicksilver, and likewise when he, *some days afterwards*, shewed the internal *Tubuli* filled in the same manner. Both were exhibited in one and the same course*.

“ So far as relates to what was done in public, I do attest the truth of what he has said upon this subject, in the ninth article of the Critical Review for November, 1757.

Green-street,
Nov. 24, 1758.

CHRIS. NICOLAY.”

It is time to conclude this chapter. The candid reader is left to make his own reflections upon it; he will see that it is a full answer to all the reasoning in Dr. Monro’s pamphlet, relative to this part of the dispute; and the Doctor will observe that I have been disposed to oblige him. He desired me †, *to produce the testimony of some few of the number who saw the preparation in question, in my Autumn course, for the year 1752.*

C H A P. II.

Of the Origin and Use of the Lymphatic Vessels.

THE business of this chapter is indeed a little more complex than that of the preceding; yet a reader who is conversant with anatomical and physiological subjects will find the demonstration as clear, and the testimonies as strong.

The authors of the Critical Review, in their account of Dr. Alexander Monro’s book upon the Lymphatics ‡, did me the honour of writing the following paragraph, which the reader will find necessary to be premised for understanding this part of the dispute.

“ Dr.

* This likewise is the same thing as saying that it was in the Autumn course, 1752.

† In the sixteenth page.

‡ Critical Review for Sept. 1757, Art. VIII. See Appendix, No. I.

“ Dr. Monro says, the discovery he has made with respect to the
“ lymphatics, was owing to experiments ascertained four years ago.
“ Now, Dr. Hunter has read public lectures in anatomy eleven years,
“ and, in every course, made the following observations on the lymphatic
“ veins: That whereas the most generally received opinion was, that
“ they were a continuation of lymphatic arteries; he, on the contrary,
“ believed them to be *the system of absorbing vessels*, and that they began
“ from all the internal and external surfaces of the body. This belief
“ he founded on these reasons: Every body allows, that all the surfaces
“ of the body are bibulous, or provided with absorbent vessels, by
“ which, mercury applied to the skin, collections of water in the breast,
“ belly, or in the cellular membrane, &c. are occasionally taken up,
“ conveyed into the circulation, and strained off again by secretion.
“ That the lymphatic veins perform this office, seems probable, from
“ the following remarks: I cannot inject them, as other veins, by filling
“ the arterial system; so that, in all probability, they are not continua-
“ tions of the arteries. I have sometimes observed in injecting, that
“ they were immediately filled with wax, when the arteries burst, and
“ the wax was effused into the cellular membrane. This looks, as if
“ they took their rise from those cells, like the veins in the spongy part
“ of the *Penis*. If they were continuations of arteries, why should
“ they be so plentifully provided with valves, which are not found in
“ the other veins of the *Viscera*? But the most striking argument, is the
“ analogy between the lymphatics and lacteals: these two systems are,
“ to all appearance, the same in their coats, in their valves, in their
“ manner of ramifying, in their passage through the lymphatic or con-
“ globate glands, and in their termination, viz. in the route of the
“ chyle. As they are perfectly similar, in every other respect, we must
“ suppose them to be so in their origin and use. The lacteals are known
“ to begin from the surface of the intestines, and to be the absorbents
“ of those parts. There is no difference but the name. The same
“ vessels are called *lacteals* in the intestines, and *lymphatics* in the other
“ parts of the body. This doctrine explains the use of valves in the
“ lymphatics. In other veins, whether large or small, the fluid is sup-
“ posed to move onwards by an *impetus* received in the arterial system;
“ but the case is not the same in vessels that suck up a fluid from a
“ surface. These require valves, that every lateral pressure upon them
“ may have the effect of an impulse at the beginning of the canal, in
“ driving the fluid onward towards their termination. This doctrine
“ of

“ of the lymphatics is farther confirmed by the absorption and progress
 “ of the venereal poison. The lacteals were discovered, traced, and
 “ their use ascertained from the circumstance of a manifest and parti-
 “ cular colour in their contents, upon some occasions at least. We have
 “ not the same advantage, with respect to the lymphatics; but, in them,
 “ what we cannot trace with the eye, we find out by the effects of this
 “ poison. We know from observation, that this *virus* may be taken in
 “ at any particular part of the body, and thence diffuse itself over the
 “ whole constitution. We must suppose it absorbed by the same
 “ vessels that absorb its antidote mercury, or any thing else, that is
 “ carried into the mass of blood by absorption. These things being
 “ of a more inoffensive nature, pass unobserved: but this poison, from
 “ its irritating and destructive quality, is apt to raise disturbance in its
 “ passage, before it reaches far enough to mix with the blood. Hence
 “ the lymphatic glands, through which every absorbed liquor must
 “ pass, are so often the parts first affected by the venereal taint, when
 “ it is spreading its contagion through the constitution. This is the
 “ theory of the venereal *bubo*. If the infection be received in the most
 “ common way, the bubo happens in the groin, because the lymphatics
 “ of the genitals pass through the inguinal glands; but, if the in-
 “ fection be received at the hand, (a case that sometimes occurs) the
 “ *bubo*, for the like reason, is formed in the arm-pit: when the disease is
 “ communicated by the lips, the glands of the neck inflame and tumify.

“ This is the very essence of Dr. Monro's treatise; and these obser-
 “ vations have been publicly made by Dr. Hunter to his pupils, for the
 “ space of eleven years, &c.”

This piece of criticism occasioned an anonymous letter being sent to
 the authors of the Critical Review*, which Professor Monro jun. tells
 us was written by his father†: upon which I was desired by those
 gentlemen to furnish them with a concise account of the facts relating
 to the controverted point. I drew up such an account, and they publish-
 ed it with the abovementioned anonymous letter, in the Critical Re-
 view for November 1757‡. It was as follows, “ Ever since I first read
 “ anatomical lectures, in 1746, among other things a little out of
 “ the common way of thinking, I have advanced the doctrine of the
 “ lymphatics being the system of absorbing vessels, and have supported

“ my

* See Appendix, No. II.

† See his note at the bottom of the second page.

‡ See Appendix, No. III.

“ my opinion by such arguments and experiments as are mentioned in
 “ your Review of last September. This appears by the MS syllabus
 “ of my lectures, which I have used in public from the beginning;
 “ by many MSS of my lectures in the hands of those who have studied
 “ with me; and by the general testimony of those who have done me
 “ that honour. I have many vouchers in my possession from gentlemen
 “ who have attended my lectures, and I appeal particularly to the
 “ following gentlemen, who are all professors or readers of anatomy
 “ now living; Dr. Collignon, professor of anatomy at Cambridge;
 “ Dr. Smith, reader of anatomy at Oxford; Mr. Hamilton, professor
 “ of anatomy at Glasgow; Mr. Cleghorne, reader of anatomy at Dub-
 “ lin; Mr. Watson, reader of anatomy in London; Mr. Galhie, of
 “ London, and demonstrator or dissector for the professor of Cambridge.
 “ So that the fact of my having taught this doctrine, and supported it
 “ by such arguments, for a number of years at my public lectures,
 “ cannot, I think, admit of a dispute.

So I thought, and so I suppose every reader, but the two Edinburgh
 professors, will think. Now, to remove all possibility of doubt upon
 this subject, I shall subjoin some passages of letters sent to me upon this
 occasion, and for the most part taken literally from MS notes, written by
 gentlemen for their own use, while they attended my lectures. I would
 not waste the reader's time, by printing their letters at full length, but
 shall extract just so much, as to shew that every argument, which I
 alluded to in the preceding history, was not only delivered by me at
 public lectures, but *is now to be found in manuscripts written from my
 lectures, before Dr. Alexander Monro pretends to have taken up the
 thought, or made the discovery*: for it was not till (or after) the summer,
 1753*, that he received “ the first hint, that the lymphatics were not
 “ continued from the arteries; but that they came from the cellular
 “ membranes, and consequently were absorbents †.”

In

* See page 22, 23, and 24, of his pamphlet.

† Shall we call the year 1753, *fortunate* or *unfortunate* for Alexander Monro, jun. Pro-
 fessor? Surely it was a *remarkable* year. He was then a *student* of anatomy, and in *that one*
year made *three discoveries*; viz. he filled the *Tubuli Testis* with quicksilver, found out that the
 lymphatics were absorbents, and saw the orifices, and introduced bristles into the ducts of the
 lachrymal gland in the human body. If he goes on at the same rate, he will become a pro-
 digy. But it was rather *unfortunate*, that Dr. Hunter should have done, and publicly taught,
 the very *same three things* before that time; and that he should be able to prove that a MS of
 his lectures was *at that time in the hands of students at Edinburgh*, in which there was some-
 thing more than a *hint* about the lymphatics, as we shall see hereafter.

In justice to the gentlemen, who have favoured me with the following extracts, I must beg the reader to remember that they were not written for the press, but as memorandums for private use: therefore the most ample allowance is to be made for the dress in which they appear. One of these gentlemen, after transcribing what he finds in his notes, says, "I hope you will excuse any inaccuracy in these notes, as you well know how difficult it is for young anatomists to carry off the particulars of any lecture, in which both their ears and their eyes are employed." Another says, "This is all I can find in my journal, relative to the topics mentioned in your letter; it is very possible, I may have misapprehended you in some points, and omitted material arguments to prove others, as I was absent some days, and my notes were all taken from memory after lecture, and thrown together in a hasty careless manner." In the last place it must be supposed, that my lecture was not always precisely the same. The longer I considered the subject, I spoke with more firmness, and collected more proofs of the doctrine; and as I always spoke principally from memory, we may imagine the arguments, brought at different times to support it, were not always the same, nor ranged in the same order.

Mr. Symons of Exeter, surgeon, attended my first course of lectures, in 1746, and the three following courses, and likewise assisted me in dissections. He writes to me from Exeter, June 13, 1759, thus: "I shall endeavour from recollection, and the assistance of notes taken at your lectures, to set forth the doctrine you taught, concerning the lymphatics, the two first years you read.-----and then told us you thought the lymphatics were absorbent vessels, and that valves are necessary where there is no propelling force.----When speaking of the *Testis*, you told us if we made a hole, through the *Tunica Albuginea*, broke or bruised the *Tubuli*, we might distend the lymphatics by blowing into its substance*. You assured us that the *Receptaculum*
" *Chyli*

* This experiment I learned from Dr. Nicolls, when I attended his lectures, and I have still a preparation of a horse's *Testis* begun by him and finished under his direction, in which a vast number of lymphatics are filled by this method. Mr. Westbrook, of Dartmouth-street, very well remembers the preparation. He was present and assisted when it was made; so was Mr. Young, surgeon, of Soho-square. I afterwards filled the lymphatics in a calf's spleen by extravasation. These things first made me think they were as to origin and use like the lacteals, which they resemble so much in other circumstances: and this opinion was every day more and more confirmed by innumerable anatomical experiments and observations, in which I was then wholly employed. The absorption and progress of poison as a
proof

" *Chyli* might be filled, before the separation of the *Testis* from the body by the above method."

Dr. Collignon, professor of anatomy at Cambridge, attended my lectures in the beginning of the year 1747, but not till the course had been some time begun. In his letter to me from Cambridge, Nov. 13. 1757, in answer to a paragraph in my letter to him about this disputed doctrine, he says; "I always imagined, that the doctrine of the lymphatics, as described by you in the third paragraph, and which I held as orthodox, was delivered by you at your lectures; but the length of time elapsed since I had the pleasure of attending them, my having omitted to take any notes, and the frequent intercourse which I have had with many of your pupils for some years, make it impossible for me to recollect with certainty, whether I first had it from your own mouth, or collected it from succeeding pupils."

Mr. Hamilton, professor of anatomy at Glasgow, who attended my lectures in 1748, and again in 1749, in a letter to me, dated Glasgow, Nov. 21. 1757, says; "From what I heard in your lectures, and from some conversations with your brother, I learned the method of demonstrating the lymphatics by blowing into the excretory ducts of the glands and substance of the *Testis*. I have by me a preparation, which I had from your brother, where, from an extravasation, a number of vessels are filled, which appear to be lymphatics, and which I keep to demonstrate them. Your demonstration of their similarity to the lacteals, I have adopted since I read here; though always with the candour of owning you as the author of that, and a number of other things."

Mr. Watson, reader of anatomy, and surgeon to the Middlesex hospital, has favoured me with a long letter on this subject, dated

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Aug.

proof of this doctrine first came into my mind upon reading what Mr. Freke says upon the cause and cure of the venereal *bubo*, sometime after the publication of his Art of healing in 1748. I made enquiry among surgeons that were much employed in venereal cases, and in inoculation; and all their observations confirmed my hypothesis. From that time, I used the argument drawn from the absorption of poisons. In the spring, 1753, I attended a case with Dr. Pitcairn, which confirmed this doctrine, as I observed to him at the time: And much about the same time, Dr. Macaulay had a very painful inflammation in his hand, from scratching his finger in opening a morbid dead body; a red and painful line ran up from his finger towards the armpit, and there terminated in a painful swelling: he asked me, what I thought it was, if a nerve, or what? My brother and I, upon seeing it, gave our opinion, that it was a lymphatic vessel inflamed by the poison which it had taken in. The Doctor remembers this circumstance particularly well.

Aug. 27. 1760, of which I shall give such parts only as are directly to the points in question.

“ ---- I keep to no particular dates, as not being material; since
 “ from my first attending your anatomical lectures in the year 1748, to
 “ within a year or two I believe of Dr. Monro jun. his attending them,
 “ I have heard you deliver yourself, over and over again, sometimes
 “ in a shorter way, and sometimes much more fully on the same sub-
 “ ject.----You spoke indeed at first with all that modesty a man would
 “ do, who thought he had reason to disagree with the common received
 “ opinion, and you grew more positive only as you had stronger
 “ conviction.

“ ----You could not say what was their (*Lymphatics*) precise be-
 “ ginning.---You believed they were not continuations from arteries,
 “ but *a particular system of vessels by themselves, the true absorbents*. In
 “ some succeeding courses, you declared you was fully convinced of
 “ this, because you had never been able to inject them. If continuations
 “ of the arteries, why not inject them, since we do inject the lymphatic
 “ arteries?---You never could fill them by injecting the artery, till you
 “ had made an extravasation in the cellular membrane. You thought
 “ them the *absorbing vessels*, because they have valves, as the lac-
 “ teals have, which are known to be *absorbing vessels*.---Why should
 “ they (*Lymphatics*) have valves in the *Viscera*, when the veins have
 “ none? Which you answered by observing that lacteals have no *im-*
 “ *petus* from the arteries, as the veins have; therefore they are fur-
 “ nished with valves, and for the same reason these lymphatics have
 “ valves.----You particularly desired us to attend to what happens with
 “ regard to the progress of the venereal poison.---It was readily ab-
 “ sorbed by the *Penis*, and conveyed to the first neighbouring
 “ lymphatic gland: thus from the *Penis* to the *Inguen*: but if re-
 “ ceived by the finger, it would then be as readily conveyed to the
 “ axillary glands; these being the two places, where the *Bubo* is
 “ commonly formed.

“ You told us---You had traced the lymphatics from the *Testis*
 “ of a dog into the thoracic duct*.---You informed us, the lymphatics
 “ might

* The reader will better understand the reason of this observation, when I have told him that, in shewing the lymphatics distended with air in the horse's *Testis*, I used to say that in the preparation indeed it might be doubted, whether they were lymphatics or veins; but that I was certain they were lymphatics, because I had inflated them by the same method.

“ might be demonstrated either by blowing into the artery, or by
 “ making a ligature on the emulgent vein in a living dog; for in each
 “ way they will be distended: but you observed at the same time,
 “ that these experiments were no proofs of their being continued from
 “ arteries, as the case might be the same as in injecting.---You pro-
 “ duced before us, a preparation of the *Testis* from a horse, in which the
 “ artery was injected red, and the lymphatics dried hollow, distended
 “ with air, which you told us had been thrown into them by inflating
 “ the cellular membrane.---I think I could recollect some few more
 “ particulars upon this subject: but I shall give nothing from memory,
 “ not caring to trust to it, at this distance of time. What I have here
 “ said is a mere relation of facts, extracted from the notes I have taken
 “ at your lectures.”

Dr. D'Urban, in his letter to me from Richmond, Nov. 12. 1757, says, “ I have been looking into the notes I made from your lectures, in the beginning of the year 1749, and---shall transcribe the paragraph just as it stands.

“ Lymphatics] A preparation of the lymphatics of a horse's *Testis**,
 “ ---I do not believe them continuations of the seriferous arteries, but
 “ absorbent vessels placed in every interstice of the body, which take up
 “ any fluid, thrown into the *Abdomen* or any other cavity; as is seen
 “ from daily experience. I have injected the spleen, which is full of
 “ lymphatics, with the most subtle injections, filled every branch of
 “ the artery or vein, when after tying the vein, and forcing the in-
 “ jection till the vessel burst, immediately on the extravasation the
 “ lymphatics became filled. This I have tried more than once, with
 “ the same success. Hence I conclude they are not a continuation of,
 “ nor have any communication with the arteries †”.

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Dr.

method in a dog, when the *Testis* was not separated from the body, and traced them all the way up to the thoracic duct.

* The preparation above-mentioned, where the lymphatics are filled by blowing into the substance of the *Testis*, and which I have generally produced at my lectures both as a specimen of those vessels, and as a proof of the doctrine. The reader is desired here to observe, that I had then a preparation, and shewed it, of the lymphatics of the *Testis* filled in this manner; and Professor Hamilton had from my brother a preparation of the lymphatics filled by extravasation. But, notwithstanding all this, Professor Monro has the modesty to assert, (pag. 43.) that “ Dr. Hunter never had made any such experiments or preparations, nor even imagined the thing possible.”

† This gentleman (Dr. D'Urban) studied at Edinburgh, in the winter 1752-3, after having attended my lectures, was acquainted with Alexander Monro jun. then a student,
and

Dr. Smith, reader of anatomy at Oxford, gives me the following extract from notes taken at my Autumn course of lectures, 1750. "If you blow or throw water into an artery or vein in the liver or spleen, &c. you can raise the lymphatic veins: hence also they were thought to be continuations of the lymphatic arteries; but I doubt it, for on injecting the spleen, I could not throw the injection into the lymphatics

and received *particular civilities* (as he expresses himself in his letter to me) from Professor Monro senior. The reader might now admire the closeness and neatness of Dr. Monro's reasoning, in his 35th page, &c. He there "evidently proves" that what I said so strongly in Dr. D'Urban's presence, in the beginning of the year 1749 was "gleaned from his Inaugural Dissertation" in the year 1755. The Professor writes thus, page 35, "But I shall evidently prove, that before that time, (viz. January, 1756) he never made the most material remarks, and the only ones which lay the ground-work for a just and allowable conclusion; but that he gleaned them from my *inaugural dissertation*, which I presented to him on my coming to London.—Such are the two first experiments, with which he sets out, by means of which only, what had passed for positive and direct proofs of lymphatic arteries, can be refuted.

"That the lymphatic veins perform this office (viz. of absorption,) seems probable, says Dr. Hunter, from the following remarks.

"1. I cannot inject them as other veins, by filling the arterial system; so that, in all probability, they are not continuations of the arteries.

"2. I have sometimes observed in injecting, that they were immediately filled with wax when the arteries burst, and the wax was effused into the cellular membrane. This looks as if they took their rise from these cells, like the veins in the spongy part of the Penis."

Let the reader, for the sake of amusement, compare these two arguments with Dr. D'Urban's and the other testimonies, and then read Dr. Monro's book from page 36 to page 43, and he will see by this specimen, what sort of a professor we have to deal with. "These experiments therefore being fully explained and insisted upon in my *Inaugural Dissertation*, (viz. in 1755.) which the Doctor had perused, it is possible he might have first learnt them from it," (very possible indeed to have extracted the knowledge of 1749 from that of 1755.) "That he did collect them in this way only, and never had made or imagined any such experiments before, the sequel does not allow us to doubt. In the first place, the Doctor's dry manner of relating them, &c." After a chain of *sensible and correct* reasoning, he concludes thus, (page 45.) "It is therefore most evident, that Dr. Hunter never had made any such experiments or preparations, nor even imagined the thing possible: and consequently he first learned from my *inaugural dissertation*, and from the one I published (afterwards) at Berlin, that the common experiments offered as direct proofs of lymphatic arteries could be refuted by experiments. Hence he is, in this respect, not only guilty of a self-convicting plagiarism from me; but, by attempting to turn my own experiments and words against myself, as stolen from him, has added an abuse to injury."

Give us leave to add, that by the same reasoning *it is most evident*, that our possessor is not only a close and clear reasoner, and a fair and candid enquirer after truth, but an elegant and delicate writer.

“ phatics till the artery was ruptured, and then it got into the lymphatic vessels. Besides, why small valves in the lymphatics, if veins, when other veins of the *abdomen* have none? They seem to be absorbents of a fine fluid, to be conveyed into the receptacle of the chyle, for dilution, &c. We find the lacteals, which are allowed to be absorbents, have valves; and the reason is, that on the least motion, the progressive circulation may be accelerated. The lymphatics therefore seem to begin from cells and surfaces.” His notes upon the lymphatics of the *Testis*, run thus, “ Dr. Nicholls cut through the *albuginea* into the substance of the testicle, and blowed with a blow-pipe: He raised the lymphatics along the testicle, and running up the spermatic rope; and as a proof that they are not veins, *I have traced them in a dog up to the receptacle of the chyle.*”---He adds, “ That I was quite satisfied with your account about the nature and use of the lymphatics, and therefore taught it ever since I began reading here, is not to be made a question. The first course of anatomy that I read for myself, began November 21, 1753.”

Mr. Davenport of Norfolk-street, attended my course of October 1751, and several succeeding courses. He gives me the following literal transcript of notes, which he took down in that first course.

“ Some assert, that these lymphatic vessels also originally take rise from the extremities of arteries; which, they tell you, may be proved in the dead subject by tying the vein, and then inflating the artery, as they enter the spleen or kidney, for instance; or in the living, by making your ligature; either of which methods, say they, will make these vessels very conspicuous. But, both these experiments are fallacious, and prove nothing; for this appearance never comes out in either, till the air or fluids have made themselves a passage, by destroying other tender parts that naturally opposed them *-----The testicles have also
“ lymphatics,

*Professor Monro (pag. 20, 21.) by way of introduction to his subject, tells us the arguments by which former anatomists were persuaded that lymphatics were continuations of arteries (particularly air, water, and quicksilver, passing readily from the arteries into them) and then adds, “ Without therefore accounting in some other way for these experiments, and refuting the arguments drawn from them, to propose a contrary opinion as a remarkable discovery, is certainly betraying a very weak and precipitate manner of hurrying to conclusions, contradicted by premises.” Dr. Hunter, as we see by this evidence, accounted for these experiments in another way, viz. by extravasation. Dr. Monro did the same several years afterwards. I will trust the reader for seeing the force of this; and now beg of him to read Dr. Monro from p. 50 to the end of that chapter: but lest he should not be able to give himself that entertainment readily, from not having Dr. Monro's pamphlet immediately at hand, I will do

“ lymphatics, though too small to be demonstrated otherwise than by
 “ Dr. Nicolls’ experiment of inflating them with air, conveyed by a
 “ blow-pipe.”

He tells me the following was an additional note, taken at a subsequent lecture, to the best of his recollection and belief in the year following, viz. 1752.

“ As the veins called Bartholine’s lymphatics are too minute to be
 “ traced to their origin, anatomists are not agreed from whence they
 “ arise. The generality take them to be continuations of small arteries;
 “ and their proofs are by no means trivial, if it be true, (as it is asserted),
 “ that by making a ligature upon the vein proceeding from any gland,
 “ you will see the lymphatic vessels of that gland greatly distended, as of
 “ the kidney for example. And they tell us too, that by driving air into
 “ an artery in the dead subject, you inflate these veins; and thus demon-
 “ strate the truth of this doctrine. Notwithstanding these specious ar-
 “ guments, however, Mr. Hunter is clearly of opinion, that these veins
 “ are rather the absorbents arising from all the different cavities of the
 “ body, as the *vesica urinaria*, *fellis*, *vesiculæ seminales*, &c. &c.-----A
 “ strong presumptive argument at least in support of this notion, is the
 “ close analogy they in many circumstances bear to the lacteals; for these
 “ rise like the lymphatics, have little else than the kneading motion in
 “ respiration to promote the progress of the chyle, and are furnished with
 “ valves at due distances, that the chyle may be always sure of passing
 “ on to the *receptaculum chyli*, and *ductus thoracicus*.-----The same place
 “ of termination have all the lymphatics; in like manner are these pro-
 “ vided with innumerable little valves; their coats are thin and fine as
 “ the lacteals; they anastomose frequently like them; and in short, seem
 “ in

do him the pleasure of transcribing one paragraph (from page 53,) which in every line shows an ease in writing, and a firmness of mind, almost beyond conception. “ I have, however, “ *clearly proved*” says the professor, “ that Dr. Hunter never had shewn any preparations, “ nor made experiments on the lymphatics, from which any conclusion relating to their “ origin could possibly be drawn: and that, so far from *pretending* to explain or refute the “ experiments of *Nuck*, *Cowper*, *Lisser*, &c. in proof of lymphatic arteries, he never so “ much as *mentioned* them. Consequently Dr. Hunter’s pretensions to even the smallest “ share of what he is pleased to call,” (which by the bye I never did) “ *the important disco-* “ *very, that the valvular lymphatic vessels are a system of absorbent veins*, are evidently founded “ on a *declaration* or conclusion contradicted by premises. And this conclusion, so far from “ meriting praise, can only be said not to deserve censure, on the supposition, that he was “ ignorant of what had been done on the subject.”

“ in nothing to differ from each other, but in the nature of the fluids
“ they convey, &c.”

Mr. Cleghorne of Dublin, reader of anatomy, who attended my lectures in the beginning of the year 1751, sends me the following extract from his notes.

“ The Lymphatic Veins.

“ Wednesday, Jan. 23. The origin commonly assigned to the lymphatics seems to be false.----Small veins that arise from corresponding arteries have no valves, the lymphatics numerous valves, (though not at such regular distances as authors paint them). Hence, it is probable they are made up of the small absorbent veins that come from the different parts of the body; and therefore have the same occasion for valves as the lacteals, &c.----

“ N. B. The lymphatics demonstrated in a calf's milt, by cutting the external coat and throwing in air.

“ Thursday March 7. The lymphatics appear in livers long kept, being distended with air generated by putrefaction.”

Mr. Davies of King-street, Covent-garden, surgeon, says, “ The following is a literal extract of the notes I made when I had the pleasure of attending your course of lectures January 1750-1; speaking of the lymphatics.-----According to Mr. Hunter, their origin more probably from all the cavities and interstices of the parts of the body.-----From the common method of raising them in the spleen and testes-----from analogy with the lacteals.-----Hence Mr. Hunter will have them to be the absorbents.

“ As this was the first course of anatomy that I had attended, I was not able to write my notes so fully; in the next course (to wit, in October 1751) I find that, upon the subject of lymphatics, I have been a little more particular.----My notes begin thus: There are two kinds of lymphatic veins: the one kind, are those in which the lymphatic arteries terminate, and whose contents go with the blood, as those of other veins, into the heart; the other kind are the lymphatics of Bartholine, which are, &c. After the description of them in my notes, comes this paragraph: Mr. Hunter's own private opinion, that these lymphatics of Bartholine are nothing but the true absorbents of the body; and that they have their origin from all the cavities, surfaces, and interstices of the parts of the body: His reasons are, 1. the common method of raising them in the spleen and testes with a blow-pipe, which could never raise the other lymphatics.-----2. These lymphatics of
“ Bartholine

“ Bartholine have valves in the *viscera*; whereas, no other veins in the
 “ *viscera* have valves.-----3. These lymphatics are never injected from
 “ the arteries.-----4. The analogy they bear to the lacteals, which
 “ have also valves.-----5. These vessels end for the most part, (if not
 “ all of them) in the thoracic duct or route of the chyle; whereas, the
 “ other lymphatics end in the blood vessels.

“ After your course of October 1751, I attended several succeeding
 “ courses, but was so much engaged as house-pupil at the Lock-hospital,
 “ that I had not time to write; but, I remember well your explaining
 “ the venereal *bubo*, upon the principle of absorption by the lymphatics,
 “ and your asking me one night after lecture, if I did not find
 “ your doctrine confirmed by the different seats of the *bubo*, in the patients
 “ of that hospital.”

Mr. Galhie of Spittle-square, says, “ As I attended your lectures so
 “ early as the year 1751, I shall, agreeable to your desire, transcribe from
 “ my notes, what you then declared as your opinion, with regard to the
 “ origin and use of the lymphatic veins. That they were not continuations
 “ of the arteries, as generally believed, but absorbing vessels, beginning
 “ imperceptibly from the different cells and surfaces of the body, you
 “ were fully persuaded, 1. Because you could not inject them as other
 “ veins, by filling the arterial system. 2. In injecting, you sometimes
 “ observed, that they were suddenly filled when the arteries burst, and
 “ the injection was effused into the cellular membrane; a circumstance
 “ which you imagined sufficiently explained, and refuted those anatomists
 “ who pretended to have filled the lymphatics directly from the arteries
 “ or veins: 3. As a farther confirmation of this doctrine, you mentioned
 “ the valvular structure of the lymphatic vessels, not observable in other
 “ veins of the *viscera*; the great likeness of the lymphatics to the
 “ lacteals in their coats, valves, course, and terminations; and lastly,
 “ the absorption and progress of the venereal poison, and inoculated
 “ matter of the small pox.”

Dr. Abernethie of Edinburgh first attended my lectures in the Spring,
 1752. The extract which he has sent me from his notes runs thus:
 “ Lymphatics (says the MS.) are of two kinds; one, which carries the
 “ thinner part of the blood back from the arteries, and so are continuations
 “ of the lymphatic arteries; the other absorbs the lymph, which is
 “ separated in all the different parts of the body for the lubrication
 “ of the parts, and conveys it into the constitution, or vascular system,
 “ through the *Receptaculum Chyli* and thoracic duct, or into the liver,
 “ and

“ and so are properly the absorbents of the body. In the absorbents
 “ are many valves irregularly placed, and at different distances; and in
 “ all respects they are like the lacteals, thin transparent vessels. The
 “ absorbents do not arise as the first kind of lymphatics just now men-
 “ tioned, but from the interstices of the fibres in all parts of the
 “ body*.”

I shall add the testimony of Dr. Hadley, physician to St. Thomas's hospital. The paragraph upon the lymphatics in his manuscript is this.

“ Lect. 4th. Jan. 31, 1754. The lymphatics are so small, and full of a
 “ liquor so colourless, that their beginnings are difficult to be found out.
 “ They are most visible in the spleen of a calf.

“ There are two sorts of lymphatic veins, the one merely the conti-
 D “ nuations

* That students of anatomy were constantly going from London to Edinburgh, and from Edinburgh to London, and giving accounts of their studies, and of what was passing at one place to their fellow-students at the other.—That it has been a custom with the more studious to write notes of the anatomical lectures, and to lend their manuscripts to be perused or transcribed by others, &c. are things so notorious, that I need not prove them.—That the professor's son, who was educated with a view of being himself a professor of anatomy, who was to be sent to London to do me the honour of attending my lectures; and was intimate with many who had attended them, and with some who had manuscripts of them, which they lent out among the students at Edinburgh;—I say to suppose that in these circumstances, he had not the curiosity or opportunity to know any doctrines or improvements that were believed to be peculiar to me, would be a *very good natured* supposition in the present case. Upon enquiry I found, that manuscripts of my lectures were very common among the students at Edinburgh, about the time that Professor Monro pretends to have made his discoveries: I was told of two in particular, one written by Dr. Alexander Bruce, who had attended several courses with me, and who by these means had got it pretty complete; the other was Dr. Abernethie's. Dr. Bruce was gone to Barbadoes, so that I had no opportunity of learning what it contained, or to whom he lent it at Edinburgh. I wrote to Dr. Abernethie upon this subject, and in answer he sent me the extract upon the lymphatics as above; and with great candour told me he lent his manuscript at Edinburgh to Mr. Mackbane, for whom part, if not the whole of it, was transcribed by his father, in 1755; and, if he was not mistaken, he lent it to Mr. Greenhill in the winter 1753-4; and he has some notion that Dr. Palmer or Dr. Amory had the use of it for a little while the same year. So that as it is plain there was a very good opportunity, I think the young professor must for his own credit plead some unaccountable *incapacity*. His brother says (a), “ I dissected five years for my father, from 1745 to 1750, and had in the winter constantly numbers of the pupils about me, and was intimate with many of them, especially of the English young gentlemen, yet while I remained at Edinburgh, I never could know one thing Dr. Hunter was doing.”

(a) Critical Review for Dec. 1757, page 525.

“nuations of the lymphatic arteries, and of course carrying the same
 “fluid contained in those arteries: the other sort terminate only in the
 “*Receptaculum Chyli* and thoracic duct. But these last seem only absor-
 “bent veins---they are called Bartholine’s lymphatics---they will run
 “a great length without an *Anastamosis*---they are traceable from the
 “testicle of a dog to the thoracic duct---they are very full of valves.

“Mr. Hunter’s conjecture of them is, that they are the absorbent vessels.
 “In the *Abdomen* of a dog particularly, and in an hydropical leg, their effects
 “are very visible. Their valves seem to argue likewise for this; for, con-
 “sidering the vessel as a vein, the valves seem useless, but as an *absorbent vein*,
 “(i. e. granting that they do not rise from the arteries) absolutely necessary,
 “as there is no *impetus* from the arteries to drive the fluid forwards.---The
 “lacteals begin in the cavity of the intestines, and run to the receptacle
 “of the chyle. These are absorbent vessels: when dry they are not
 “distinguishable from the lymphatic veins.---In the progress of the ve-
 “neral poison, whenever it gets into the constitution, it appears first in
 “some of the lymphatics, situated near the place where the infection
 “is received; if (e. g.) it is received at the *Penis*, it will appear in
 “the glands of the groin, if, by any accident, in the hand, the *Bubo* is
 “formed in the *axilla*.”

If there be any reader, who is not satisfied with these testimonies, I
 must suppose, either that he does not understand the subject in dispute,
 or that he is resolved not to be satisfied. I will therefore conclude this
 chapter, and leave Dr. Monro in the full possession of all his reasoning.

C H A P. III.

The HISTORY of the DISPUTE.

IN the two preceding chapters, the reader has been informed what Dr.
 Hunter had done on the subject of the *Testis* and lymphatics; pre-
 viously, not only to Dr. Monro’s publications, but to the times when he
 himself says he first made the discoveries. Let us now see when, and
 upon what occasion, Dr. Monro published these discoveries, the rise and
 progress of the present dispute, that the reader may be enabled to pass his
 judgment according to the evidence of facts.

Dr.

Dr. Monro published his account of the injection of the *tubuli testis* in the Edinburgh *Essays and Observations*, in 1754. Dr. Hunter took no other notice of this than by declaring to his pupils, that he had shewn these *tubuli* injected at his lectures, before Dr. Monro pretended to the discovery.

In the summer, 1755, Dr. Monro published a *Thesis*, (*De Testibus in var. animal.*) in which that discovery was improved and extended. In the 12th chapter of this *thesis*, after some observations made upon the lymphatics of the *testis*, he says *: “ I have explained these experiments at greater length, “ as they first incited me to try others on the lymphatic vessels in general; and, as I have found that these could not only be filled from the “ excretory ducts of the glands, but likewise in a manner not hitherto “ remarked by authors, viz. by an effusion of fluids into the cellular “ membranes and cavities of the body, of which I have already given “ several examples; and that, without an effusion into the cellular membranes, they never, in my experiments, did admit liquors injected “ into the blood-vessels to enter them: these, among other things, furnished me with arguments of no small weight to prove, *That the vular lymphatic vessels, through the whole body, were a system of absorbed veins; and that they did not proceed from the branches of arteries, as is the common opinion.* But at present to propose all that might be “ disputed upon this subject, would far exceed the bounds of such a dissertation; and it will be much fitter to treat of them apart, viz. of “ their origin, fabric, manner of acting and use, when my time shall “ better permit.”

Dr. Donald Monro soon after presented me with this *thesis*, and the author's compliments, telling me he was coming up to London to attend my lectures. I looked it over at my leisure, and must own the above-mentioned passage struck me, and gave me some suspicion that he was going to treat me unfairly about the lymphatics. However, he was the son of my old master, he bore the character of an ingenious young gentleman, he was appointed conjunct professor of anatomy at Edinburgh, and his coming to study with me was surely a particular honour conferred upon me; I therefore wished from my heart to procure his friendship, and thought it my duty to receive him kindly, and with respect. It was very natural to wish all this upon my own account, as it must be agreeable to any man who reads anatomy in London, to stand well in

* I have given his own translation of the paragraph from the 36th page of his pamphlet.

the opinion of a professor of anatomy in a school of physick, which does so much honour to this country. Besides, I thought that if a youthful eagerness for reputation, flattered by paternal partiality, had misled him in any point with regard to me, I should be more likely to set him right, by a friendly and open behaviour.

As I had conceived such hopes, the reader may imagine what kind of reception I gave him. I told him, that Professor Monro's son had a right to command any service I could do him in the prosecution of his studies; and it gave me some pleasure to think, that I might be of some little use to him. Sir, said I, you mean to devote yourself to anatomy, and to teach it. You will therefore wish for every possible advantage. In London we have commonly a greater plenty of subjects than at Edinburgh, and for that reason perhaps have made some progress in the practical part of anatomy; particularly in the arts of making preparations *. In the dissecting room you will find a great deal of that sort of work going on through the whole winter, under my brother's direction. If you can make any use of us, you will do us a pleasure.

After my lecture, I often spoke to him in a familiar way upon anatomical points, and never once said any thing at my lecture, that I thought he could complain of. I there related how, and at what time, I had shewn the *tubuli testis*, mentioned his having done the same thing in a variety of animals; and when I was obliged to speak of the tube, which he says he discovered rising from the *epididymis*, I did not name him, because I could not acknowledge it to be a discovery; and treated the matter tenderly, because I believed it to be a mistake. But, however that might be, when I was claiming my just right in his presence, I did not pre-

* Some readers may perhaps think this was a very free speech. It was free, but, I hope, not rude. Professor Monro sen. has acquired, and undoubtedly has deserved, great reputation in anatomy; but, in every body's opinion, he might have been a much better anatomist, if he had had better opportunities; if he could have been better supplied with dead bodies, and had been less interrupted by his private practice. Tho' he had written upon injections and preparations, when I attended him, he had almost none. In his lectures, his custom was to undervalue preparations (if I understood him right) and to insinuate that they were of no use; and indeed the very few he had were well adapted to support the opinion. In London Mr. St. Andre, Dr. Sandys, Dr. Nicholls, and some others had improved this branch of anatomy. Their methods were made public, and explained fully (with any little improvement that occurred to me) at a lecture, which it has been my custom to set apart for that purpose; and now, if I am not misinformed, even Professor Monro sen. speaks of preparations with temper and approbation, and his son has been much employed in making them.

pretend to more, and therefore ingenuously confessed, that I knew nothing of that duct.

When I treated of the lymphatics in his presence at lecture, I was pretty full upon the subject of their being absorbents *, and said I had taught this doctrine, and supported it by such arguments as I then made use of, from the first of my reading lectures. I said so, that I might put him upon his guard. And then I did him the justice to add, that I found this doctrine laid down or advanced in a general way, in his Inaugural Dissertation †. *I was not afraid of truth, and therefore did not suppress what he had done.*

After all this, he printed his treatise on the lymphatics at Berlin, 1757, and his brother soon after presented me with a copy, in the name of the author. I read it over, and was astonished to find there the hypothesis I had advanced, supported by the very arguments which I had used for the same purpose ‡, with the addition of an introduction, in which he quotes above twenty of the latest writers, to shew that the opinion was new. I say I was astonished, that he could do this without once even mentioning my name.

The reader would be astonished too, if he knew the circumstances minutely, many of which I am under a necessity of suppressing. I will take the liberty of mentioning only one little piece of private history, which

* This Dr. Monro allows in his 35th page.

* This he allows in his 37th page.

‡ He made some of the experiments indeed with quicksilver, which I had made with air, and with common injection; but the experiment was still the same, and we both drew the same conclusion from it. I speak now of air, injection, or quicksilver, getting into the lymphatics from the cellular membrane. In the argument taken from poison, I mentioned indeed only the venereal and the variolous, but meant that it might be applied to all poisons which are absorbed; as is plain from the opinion given in Dr. Macaulay's case, and mentioned in the last chapter.

I may take this opportunity of explaining upon the same principle, a symptom in venereal cases, which puzzled me for a long time, and about which I think the learned Dr. Astruc himself has not given much satisfaction. When the *penis* is affected, especially when there are sores in that part, we frequently observe a hard chord, like a piece of cat-gut, under the skin, running along that member. Sometimes there is one on each side. At first it is only to be felt at the extremity of the *penis* coming from the affected part, but it soon extends itself to the root of that organ, and frequently may be traced in the fat of the *pubes* stretching across towards the groin. From its course, it is plain, that it cannot be a nerve, artery, or vein. I have for some time been well convinced, that it is a lymphatic vessel of the part, indurated by the poison which it conveys: but I have not yet had an opportunity of being *certain* of the fact by dissection.

which I presume will be satisfactory. I knew that Dr. Black, professor of medicine at Glasgow, who gave the letter in favour of Dr. Monroe*, had studied at Glasgow before he went to Edinburgh, where I imagined he must probably have known something of my opinion about the lymphatics before he became acquainted with Dr. Alexander Monroe jun. I made some enquiry, and was informed my conjecture was well founded: then I wrote a letter upon the subject to Dr. Black, in which I proposed the three following questions; first, if he had seen a MS of my lectures at Glasgow or Edinburgh; secondly, if he knew it was an opinion of mine that the lymphatics were the system of absorbents, previously to his seeing Dr. Monroe's MS on that subject; and then, thirdly, if he had said any thing of this to Dr. Monroe when he saw his manuscript. In answer to this letter I received the following, which I shall give at full length.

“ DEAR SIR,

“ The dispute between you and Dr. Monroe has given me a great deal
 “ of concern, and I have often wished that my endeavours to prevent it
 “ had been successful. But since I was not so happy as to effect this, and
 “ that I am called upon as a witness, the only task left me is to do im-
 “ partial justice to both sides, by attesting those facts of which I have
 “ any knowledge, and concerning which my testimony is demanded.
 “ This I have already done with respect to Dr. Monroe; and shall now
 “ likewise answer those questions which you have been pleased to put
 “ to me.

“ In answer to the first question I must assure you, that I never saw,
 “ nor ever had any knowledge of any manuscripts of your lectures.-----
 “ But I must declare, in answer to the second, that I knew it was an
 “ opinion of yours, before I went to study in Edinburgh, that the lym-
 “ phatics were a system of absorbents, and therefore quite distinct in
 “ their nature and office from the vessels which belong to the system of
 “ the heart. This I learned from Dr. Cullen†; and the argument
 “ mentioned to me, according to the best of my remembrance, was,
 “ that in making injections, you had observed that the lymphatics were
 “ commonly filled when the injected matter was extravasated, and not
 “ otherwise; and, if I am not mistaken, he likewise told me, that, agree-
 “ able

* Dr. Monroe's pamphlet, page 27.

† At that time professor of medicine at Glasgow.

“ able to this opinion, you filled the lymphatics of the testicle with air
 “ by bruising its substance a little, and blowing air into it thro’ a hole
 “ in the *Tunica Albuginea*.

“ The conversation which passed between Dr. Monro and me when
 “ he shewed me the manuscript for his *thesis*, was, according to the
 “ best of my remembrance, to this purpose. As soon as I had read it,
 “ I told him that he must strike out intirely the Dissertation upon the
 “ Lymphatics, because the opinion he there proposed and supported, had
 “ been entertained by you a very long time. I even ventured to tell him,
 “ I could not help suspecting his having got a hint of it, some time or
 “ other, from me. He seemed surprised and displeased, and asserted,
 “ that it occurred to him in consequence of some *phænomena* in his ex-
 “ periments, as related in the Dissertation; that he owed it to no person
 “ whatever; and that he was resolved to publish it immediately. I in-
 “ sisted that, at any rate, if he did publish it then, it would be abso-
 “ lutely necessary for him to mention Dr. Hunter as having been of the
 “ same opinion before him, both because I thought politeness and can-
 “ dor required such a confession, and because he might expose himself
 “ to very disagreeable suspicions by acting otherwise; but advised him to
 “ delay the publication of it until he had frankly conversed with Dr.
 “ Hunter himself, whose course he proposed to attend the following
 “ winter, not doubting but that, by such a conversation, all cause of
 “ shyness and dispute would have been prevented one way or other.

“ This, Sir, to the best of my remembrance, is the substance of what
 “ passed between us upon this affair, the consequences of which have
 “ since given me a great deal of uneasiness.

“ I am, my dear Sir, with the greatest esteem,

Your most humble servant,

Glasgow,
 July 1, 1760.

JOSEPH BLACK.”

When I shewed this letter among my friends, some of them seemed to think, that, in strict justice, Dr. Black ought to have mentioned some of its contents in the letter which he gave as a testimony for Dr. Monro. I own his conduct did not require any apology with me; I was well convinced of his integrity, and approved of his benevolent tenderness. However, I wrote to him again upon the subject, and was favoured with his answer, which I think it my duty to lay before the reader.

“ DEAR

“ DEAR SIR,

“ I received your letter of the 10th of this month, and cannot oppose
“ your intention of printing my former. I am obliged to you for the
“ tenderness and delicacy with which you express your concern, lest the
“ world should find any difficulty in accounting for my conduct in this
“ affair. I confess it has been improper. When I wrote my letter to Dr.
“ Monro, I ought, no doubt, to have also declared those circumstances
“ which seem to favour your side of the question: nor do I pretend to
“ offer a sufficient apology; but you must give me leave to tell you,
“ how I was induced to act as I did. And if I disclose sentiments which
“ may not perhaps agree altogether with yours, you must forgive the
“ freedom I take, and consider that it is requisite for me to give my
“ motives for what I did.

“ I must own therefore that I was under no necessity, but moved by
“ compassion for a friend, who had taken a step which I no doubt thought
“ excessively wrong, but who was threatened with the most afflicting
“ and insupportable of misfortunes, the loss of his character and repu-
“ tation, and who, after all, might possibly suffer this loss undeservedly
“ too, through my being forward in publishing what you have lately got
“ from me. For tho’ I could not help suspecting, from the circumstance
“ of our frequently conversing together when I was in Edinburgh, that,
“ tho’ he might afterwards forget it, he might have had the first idea of
“ that opinion concerning the lymphatics from me, yet I had no reason to
“ be satisfied of this point. And that he could pick up every particular
“ of your arguments and experiments in order to publish them as his
“ own, is what I could not believe. I knew him to be acute, industri-
“ ous, and keen in the pursuit of knowledge; and believed him very
“ capable of inventing the several arguments and experiments which he
“ has published in his *Thesis* and Dissertation, but could not conceive him
“ to be capable of the other: and I must observe, that before I saw Dr.
“ Monro’s papers, tho’ I had a general notion of your doctrine upon the
“ lymphatics, I was not master of many arguments in support of it;---
“ because no doubt I had not attended sufficiently to the subject, or had
“ forgot a part of what I had heard, or had not heard the whole: but
“ this I am sure of, that I read this part of his papers with particular
“ pleasure, as finding that doctrine rendered much more probable and
“ interesting than I had conceived it before; for he did not communi-

“ cate

“ cate his thoughts upon the subject to me until he had put them together in writing.

“ When I therefore considered what he must suffer, should I add probability to the accusations with which he was charged, and considered at the same time, that I had no reason to be satisfied that he deserved such distress, I was persuaded to give at that time such part of my evidence only, as was perfectly direct and conclusive; and to reserve the rest until you laid me under the necessity of declaring the whole.

“ You desire to know in what particular manner Dr. Monro communicated to me the method of raising the lymphatics by blowing air into the glands. So far as I remember, when he said that he could raise the lymphatics in this way, he told it me as a piece of anatomical news, or as a curious anatomical fact, which I imagined was a discovery of his own, as I had not consulted Nuck or Cowper upon the subject; but I cannot say, that he either mentioned Nuck or Cowper, or said that it was his own.

“ I hope you will forgive the freedom I have taken in this letter, and believe me to be with the greatest respect,

Your sincere friend and humble servant,

Glasgow,
Feb. 26, 1761.

JOSEPH BLACK.”

These letters are so strong and clear, that they need no comment.

Upon this friendly admonition, Dr. Monro in the mean time suppressed the dissertation, and in the following winter came to London to attend my lectures. His father, Professor Monro sen. assures the public *, that, “ he went to London in absolute ignorance of Doctor Hunter’s having any particular opinion concerning lymphatics-----attended his lectures, and was surprised when he heard Dr. Hunter teach the doctrine of lymphatics being absorbents.” Mercy upon us! Did Professor Monro sen. really know how things were, and make this public declaration? He says, in the same paper, “ I thought it my duty, as a friend who knows the facts relating to the present dispute, to send you a fair state of them.”

If so, I cannot with any decency suppose but that the son must have *concealed from his father*, that he knew my opinion of the lymphatics;
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* Critical Review for November, 1757, page 432. See the Appendix. No. II. The young professor tells us (pag. 2.) that this was written by his father.

that the father was not trusted with Dr. Black's remonstrance; and that the son informed him from London of his being surpris'd to hear me teach that doctrine. This supposition excuses the father, but how does it affect the son? If he was fully satisfied that the discovery was honestly his own, as soon as he found that it might, and probably would be disputed with him, it is almost impossible to imagine he should not have consulted his father in so ticklish a situation; that, on one hand, he might not lose his just right, nor, on the other, be suspected of a low plagiarism, which to a generous mind would be still more insufferable*.

I shall now proceed in the narrative. He came to London, attended my lectures, never spoke to me upon the subject, went next winter to Berlin, and there published his dissertation; and though he knew from my own mouth, in presence of a great number of students, that I had taught the same doctrine, and supported it by the *very same* arguments several years before, he did not mention my name, not even in a marginal note.

What made Dr. Reimarus join my name to Dr. Monro's, when he spoke of this doctrine in his *thesis*? Was it not because *it was but just*, because *his conscience did not accuse him*?-----What made Dr. Monro avoid it?

The authors of the Critical Review gave an account of Professor Monro's dissertation in the Review for September, 1757, and did me justice upon the occasion. In the Review for November, 1757, they published an anonymous letter, afterwards acknowledged to be from Professor Monro sen. in justification of Professor Monro jun. and a short state of the case from me. Lastly, in their Review for December, 1757, they published a letter from Dr. Donald Monro, followed by remarks which he had given me an opportunity of making; and, at this precise time, the young professor passed through London in his return from Berlin to Edinburgh. Let us here try to guess at the state of his mind, by his behaviour on this occasion.

Had he received no civilities from me, but, on the contrary, insults
on

* If it were not to break in upon the reader's reflections, I would now desire him to consider the account that Dr. Monro has given of his thesis, (pag. 25.) and to allow it all the weight and credit which he thinks it deserves: and to consider Dr. Monro's history of his proceedings upon this subject, (pag. 23, 24, and 25.) and believe, if he can, that Dr. Monro would have said nothing of them to Dr. Black, till they were drawn up for publication, if he had been easy in his mind. Young men are not so slow in talking with their friends of their pursuits and discoveries. But if he felt any forebodings that Dr. Black might be unpropitious, it was no wonder he kept him ignorant as long as he could.

on himself and his friends, as he pretends *, and had I behaved in his absence so as to deserve the load of infamy and reproach which he presently after threw upon me in an abusive pamphlet; surely it would have been indecent meanness of spirit to shew me any respect in passing through London. Yet he tells us †, “ he called at my house, in company with his brother, and, not finding me at home, left his name with my servant, and desired him to tell me, that *he would have called more than once*, but that he was only passing through London in his way to Scotland, and was to set out next morning.”

Did he call upon me in civility, and leave such a respectful message after such my behaviour to him; and after he had been in London some days, when he must have been informed of all that had passed in his absence; and the evening before he set out for Edinburgh, where he soon after wrote the furious pamphlet against me? No body will believe it. But when I have told the reader one material circumstance about that visit, he will begin to conceive the intention. He called upon me indeed at my house in Jermyn-street, but it was *when I was reading my lecture at Covent Garden*; that is, *when he was certain I was not at home*, for he knew my hours of reading, and he knew that nothing but the most urgent business could prevent my being at lecture at the usual hour; an accident which he knew would as certainly prevent my seeing him, if I should not be at lecture ‡.

Now if the reader believes that I had been really civil to Dr. Monro, when he attended my lectures, he will see a reason for his thinking it right to shew me this respect: in the next place, if he believes that Dr. Monro had not properly returned my civilities, and had acted disingenuously

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* Page 17.

† Page 3.

‡ In this dispute, I appeal to facts, and therefore shall here annex the affidavit of my servant.

“ I, James Duncan, do voluntarily make oath, that some time last winter, (I believe it was on the 13th of December,) Dr. Alexander Monro jun. in company with his brother, called at Dr. Hunter’s (my master’s) house in Jermyn-street, while he was at his lectures in Covent Garden, at the usual hours. Dr. Alexander Monro asked if Dr. Hunter was at home: I answered no; and he left his name, and desired me to tell Dr. Hunter he had called. This was all he said to me. He did not say, that he would have called more than once, nor that he was only passing through London, nor that he was to set out next morning. When my master came home, I told him, and he said that Dr. Monro could not suppose he was at home at that hour.”

JAMES DUNCAN.”

Middlesex, to wit, sworn before me this 26th day of October, 1758. J. FIELDING.

nuously in his publication, he will see a reason for his chusing a time, when he was sure not to find me. In the last place, the reader will not perhaps err greatly, if he supposes that Dr. Monro, returning to Edinburgh, found that he would infallibly suffer in his reputation, if he did not write a bold defence of himself, and make the world believe, that Dr. Hunter had not one grain, either of knowledge, candour, or veracity. This supposition is the less improbable, as every man who has studied at Edinburgh for these thirty years last past, must know that he would naturally fall into the hands of a strenuous adviser, who has been long practised in exhibiting the characters of anatomists.

C H A P. IV.

REMARKS upon some EXTRAORDINARY PARAGRAPHS in Dr. Monro's Pamphlet.

IN the three foregoing chapters I have given the authenticated facts relating to the principal points in question, together with the rise and progress of the controversy. In these, some reflections and inferences have been made; but decision and judgment was left to the intelligent and impartial reader. It might therefore be expected that I should now have done with the subject. I certainly should, if the Doctor had kept to the points in question between us. My first and only argument was, that having been prior to him in both the improvements or discoveries, I had a right to be mentioned, in a marginal note at least, in his treatise of the lymphatics, as having been of the same opinion. This neglect was the charge that was brought against him. But, instead of a plain and candid answer, his *defence* turns out, almost in every page, a violent *accusation* of me; and he has introduced several things foreign to the dispute, as if he wrote, not to justify himself, and to get the better of the argument, but to destroy the reputation of his antagonist. It is therefore incumbent upon me, after having furnished the reader with materials for judging between us in the original dispute, to consider some of these injurious reflections, lest they should be supposed unanswerable. I shall take them in order, as they occur.

§ I. In his introduction, pag. 2, 3, and 4, he complains very much of a postscript which I had written, and pretends to think it *highly probable*, *my conscience must have rejected what my pen there affirmed, &c. &c.* The fact

fact was this: Dr. Donald Monro wrote a letter (See App. No. IV.) to the authors of the Critical Review, in defence of his brother, December 7, 1757, which they sent to me on the next day, and I immediately wrote some remarks upon it. In two days after this, Dr. Alexander Monro came to town in his return from Berlin. I then imagined that some alterations might be made, or that the paper might be recalled, in consequence of the two brothers having considered the affair together; I gave notice of this to the authors of the Review, and begged the favour of them to defer, as long as they could, printing his letter, and my remarks. On the 22d day of the same month, that is, eight days after Dr. Alexander left London, I was informed by the authors of the Review, that they had heard no more from Dr. Donald Monro, and that they could no longer defer the printing. I then dated my remarks; and, as Dr. Donald's letter was concluded with observing, that it was *doing justice to a brother who was not present to answer for himself*, I put down this postscript, viz. *Dr. Alexander Monro jun. who was abroad, has been lately in town, and we are therefore bound to believe he has approved of the steps which his brother has taken in his defence.* Was not the fact true, and the inference both natural and fair? Must not we believe that his brother told him what he had just done; and, if he did not approve, why did he not stop the proceeding? Why should my *conscience reject what my pen here affirmed*? Why of all things should it be construed *calling Dr. Donald's veracity in question*? But, as I would leave as much as possible to the reader's determination, I will only beg the favour of him now to read what the Doctor has said upon this subject in his first four pages. He will then see what I was to expect from such a spirit of misapprehension or misrepresentation.

§ II. I must next give a large quotation of four paragraphs, which are of a very singular nature, and seem to deserve the reader's attention. He says, pag. 17.

1. "The Doctor has thought proper to mention the respect and civilities he shewed me in London, with the appearance indeed of compliment; which he has, however, so misplaced, that some think it rather implies a reproach of my ingratitude*.

"Now, as I am not conscious of deserving this reproach, I shall explain to the reader the real sense of these civilities, by an example relating to the present subject; and assure him, that I could produce several

* Critical Review, pag. 438.

“veral others such like, if not to myself, to those at least in whom I must think myself interested.

2. “On coming to London I presented my *Inaugural Dissertation de Testibus in variis Animalibus*, to the Doctor. A few days thereafter he demonstrated the male organs; and, among other things, observed, that some had described remarkable vessels coming off from the *epididymis*, and affirmed that they were seen frequently*; but that, for his part, he had made a considerable number of experiments, and that he never had seen any such vessels; and that he, therefore, very much questioned if such discoveries, or rather pretences to discoveries, were much to be trusted. I don’t say these very words were used by him, but he spoke to that purpose, and in a manner which cannot well be described; but which, with his never citing Dr. Haller, plainly shewed at whom he levelled. Most unluckily, however, for the Doctor, when he handed about his preparation, I evidently saw in it one of these very vessels, as conspicuous as I had ever observed before; which I remarked to Dr. Farr, now physician at Lymington, who chanced to sit next to me, and afterwards particularly to Mr. J. Hunter, brother to the Doctor.

3. “Whilst this serves as a sample of the Doctor’s civilities and respect, it may at the same time give an idea of his accuracy in making observations, and circumspection in drawing conclusions.

4. “A proof too of the Doctor’s candour is, that, since that time, he demonstrates such vessels, and passes over in silence by whom they were first remarked, and described; or, in what way, or by whom, they were first pointed out in his own preparations to his brother, and so to himself.”

In the last chapter I have said with what a disposition I received Dr. Monro, and how I *meant* to behave to him at the lectures. If the reader believes what I said, he will easily believe what I must have felt in reading these four paragraphs. I shall examine them in order.

1. The only remark which I shall make upon the first is, that I am sorry I cannot altogether clear him from what he calls the implied reproach. But if he thought it any breach of the respect due from me to him, or to his father, that I differed from both of them in some anatomical opinions,

“* I before mentioned my having first painted such a vessel in the *Philosophical Essays* of Edinburgh, Vol. I. I had afterwards given three or four figures of it from different subjects, in my *Inaugural Dissertation*.”

ons, I grant that he must have met with offence. But why was he so unreasonable? Upon all these occasions I *meant* to be as tender as possible; and never then named either of them, that the point itself might be considered, and not the author of the opinion.

2. In the second paragraph he gives an instance of an unmannerly insult received from me at my lecture. I assure him, and the reader, upon my honour, that I meant nothing less. Those who know me will not, I hope, readily believe that I was very unmannerly before a numerous assembly to a person who behaved like a gentleman, who was known by the company to be the son of my old master, and who, by the rules of the place, was not at liberty to speak in his own defence. I do not pretend to say that Dr. Monro did not feel what he expresses. Where there is a fore, there will be some tenderness: but I declare again, that I did not mean to hurt him. I was therefore shocked at the imputation; and asked a number of gentlemen, for the satisfaction of my own mind, if they could recollect, whether what I then said had appeared to them as it seems it did to Dr. Monro. All of them assured me that there was no appearance of disrespect to the author of the opinion, that is, to Dr. Monro. Very fortunately, there happens to be the most unquestionable evidence in my favour, that the nature of the thing could admit of. It is the testimony of a gentleman of undoubted understanding and honour, who was present; and who, from some circumstances which I shall presently relate, must have been without prejudice when his judgment was formed, and who could not possibly forget what that judgment was. I speak of Dr. Warren, physician to St. George's hospital. The account which he gave me, when asked was this: he happened not to know who the anatomist was that had described the duct in question; and on the next day, while my expressions and manner were yet fresh in his memory, he was asked by a friend to Dr. Monro, whether he did not think what I then said was in a slighting way. He gave that gentleman his opinion freely, but in the negative; and says now, that he is sure his judgment was unprejudiced, because he did not know at that time who was meant; and is sure that he has not forgot, because the next day's conversation with Dr. Monro's friend made a deep impression upon his memory. I presume the reader will here make a reflection, or two; and therefore I will not disturb him with any of mine.

I come next to consider the last part of the second paragraph, viz. "Most unluckily, however, for the Doctor," &c. Some, I presume, will

will not think it very probable that Dr. Monro. with one glance of his eye, should plainly see in my preparation, when inclosed in a bottle, what neither I, nor my brother, who had injected, dissected, studied, and handled the part, knew any thing of. Dr. Monro did indeed mention his opinion about that preparation to my brother; but in a very proper manner, and received, in a very decent way, my brother's answer, viz. *That it was not a duct going from the epididymis, but a little process or projecting part of the epididymis itself.* That it was so, I am certain from several circumstances, and that such a process is no very extraordinary thing, Dr. Monro will probably know when he has had some more experience. But that I might answer Dr. Monro with facts, rather than opinions, my brother took the preparation out of the bottle before proper witnesses, examined the part by dissection, and gave me the following account: " I took the preparation in question out of its bottle, in presence of Mr. Jones and Mr. Blount, October 9, 1758, and carefully examined by dissection the little appendage of the *epididymis*, which Dr. Monro had spoke of to me. I found it to be (what I always took it for) a little part of the *epididymis* projecting beyond the rest, as a little process or lobe. I unraveled the greatest part by dissection, and observed that it was made up of the convolutions of the common excretory canal. And when it was thus unraveled, we saw plainly that this portion of the canal came out from one part of the *epididymis* and went into another; so that it could not possibly be a tube or duct going off from the *epididymis* to any other part of the body.

" What made me very clear in my opinion from the first, was, that in injecting this *testis* (the first in which the internal *tubuli* were filled with mercury) I attended very particularly to the appearance of the quicksilver, as it was running along the meanders of the *epididymis*, and observed this process fill in regular progress before any of the mercury got beyond it, or higher: and without doing any thing more than continuing the pressure, the mercury ran afterwards through all the rest of the *epididymis*, and into most of the internal *tubuli*. Now, if it had been a duct going off from the part, it was so large, and admitted the quicksilver so readily in the beginning of the process, that the mercury must have run out, or filled this duct some way up the spermatic cord, much sooner than run thro' the slender and long winding tubes at the upper end of the *epididymis* and in the body of the *testis*."

3. After what has been said, it will be unnecessary for me to make any

any remarks upon the third paragraph of the quotation from Dr. Monro. I will only beg of the reader to *make* his own *observations* and *draw* his own *conclusions*.

4. I shall now answer the fourth and last paragraph, viz. "A proof too of the Doctor's candour is, that, since that time, he demonstrates such vessels, and passes over in silence by whom they were first remarked and described; or, in what way, or by whom, they were first pointed out in his own preparations to his brother, and so to himself." The short and plain answer is, that this assertion is directly, and altogether contrary to truth and fact. I never saw such vessels in my life, (I mean any other than lymphatics) I never shewed, nor pretended to shew them in my life, either at lecture, or any where else; and I always was inclined to believe, and am so to this day, that no such vessels exist; and that Dr. Monro had taken a common lymphatic of the part for some remarkable tube, which hitherto had escaped the pursuit of anatomists. In my autumnal course of 1758, soon after Dr. Monro's pamphlet was published, I did myself justice with regard to this accusation, in presence of several gentlemen who had attended all my courses since Dr. Monro's going to Berlin; and called upon them to witness (which they readily then did) that in every course during that time, I spoke of those vessels in the manner I have just related. The only excuse I can possibly suggest for this bare-faced assertion is, that Dr. Monro was so informed. But even upon that supposition he was inexcusable for asserting it *absolutely*, as it affected another person's character so materially. He surely ought to have considered that his information might be false, either from some mistake at first, or in the conveyance; or that it might be some *malevolent misrepresentation*. If I am not misinformed, he knows instances of the last kind with regard to me. For example, he knows, I believe, that a young professor attended my lectures, and some time afterwards read lectures himself*.

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* To understand this anecdote, the reader ought to know that there is among my preparations a tongue, one half of which is injected and the other not; so that the one half is red and the other white. The line of division is strait, and exactly in the middle. The *foramen cæcum* is very conspicuous. I have often sent it round at lecture, as a specimen of that *foramen*; and judging it useful to illustrate diseases, and to stamp them upon the memory of students, by objects of sight, whenever it can be done, I used generally then to speak of the *hemiplegia*, in the following manner. *If we suppose the nerves of each side to terminate exactly in one half of the body, we see how in the palsy of one side the sensible and insensible halves will be divided by an exact middle line.*

nuous behaviour, he shewed his pupils a tongue, one half of which was injected, the other not, and said, (as I was informed) "Dr. Hunter shews such a preparation as this in his lectures to explain the *hemiplegia*;" and with a sneer added, "but I have always looked upon the *hemiplegia* as a disease of the *nerves*, not of the *blood-vessels*."

§ III. The next thing to be considered in Dr. Monro's pamphlet, is what he says of the lymphatic glands, pag. 38, 39, and 40. I should have passed it over, with some other things of the same kind, if Professor Monro sen. * had not desired to know what I had done upon this subject.

I had made no satisfactory observations upon the lymphatic glands for several years after I had read lectures, and therefore never took upon me to decide between Nuck and Ruysch, whether they were cellular or only vascular. All this, as well as the manner in which the lacteals and lymphatics pass through them, I professedly gave from authors, and not from my own observations. My brother found out, to the best of my recollection, in the year 1753, or 1754, that he could fill these glands uniformly, and the lymphatic vessels going from them, by pushing a pipe into their substance, as Dr. Nicholls had done in the *testis*. When examined in this way, they have exactly the appearance that Nuck describes. After I had seen this experiment repeated to my satisfaction, I mentioned it in my lectures, and then confirmed what Nuck had said, from my own observation. Having found out so easy a method, my brother then intended to have discovered or ascertained the structure, and, if possible, the use of the lymphatic glands; to have traced the lymphatic vessels all over the body, and to have given a compleat description and figure of the whole absorbing system. This he proposed to accomplish, as his other employments should permit. He occasionally filled these glands with air, with mercury, and with soft wax. They always appeared to be cellular, and the lymphatics to pass through them in the manner that was commonly supposed. To see more exactly how these things were, he injected some with wax, and then steeped them in spirits of sea-salt for corrosion: but he learnt nothing of them by this experiment; for in washing they all crumbled to bits, not only the supposed cellular part, but the visible branches of the lymphatic vessels: which was occasioned, as he imagined, by the frequency or number of valves in them, intersecting the column of wax. As he wished not to be anticipated, I treated the

* Critical Review for November, 1757. See Appendix. No. II.

the subject lightly at my lectures, and to the best of my remembrance, only mentioned his manner of filling the glands, and the easy method of raising the vessels wherever there are such glands, and his opinion of the thoracic duct climbing so far as the upper *cava*, instead of terminating immediately into the lower, viz. that the chyle was carried a great way before it was poured into the blood, probably for the sake of being first mixed with almost all the lymph of the body. Both these observations I made as from my brother, when Dr. Monro attended me; and when the hurry of dissections was pretty well over in the spring, my brother sat about a preparation, which he proposed as a basis for his intended description and figure of the *absorbing system*. Dr. Smith of Oxford happened to be in town at that time, and being much pleased with the intention and with the preparation, was frequently in the dissecting room while my brother was dissecting, and while Mr. Riemsdyk was making the drawing*: so were many gentlemen of our acquaintance, besides students.

In that preparation and figure, the lymphatic vessels from the ham upwards to the thoracic duct were seen, as well as the inguinal and lumbar glands, all the larger lacteals at the root of the mesentery, the *receptaculum chyli*, (or what is so called) and the thoracic duct, all I say, finely filled with mercury. So far my brother had gone. A very indifferent state of health, the effect of too much application to anatomy, which obliged him to be much in the country, other unavoidable avocations, Dr. Meckel's publication upon the lymphatic glands, and a dislike of having any dispute with Dr. Monro, which, by his father's letter in the Critical Review seemed to be threatened, all these things, I say, have from that time made him lay aside the scheme; and he will hereafter finish it, or not, as he may think proper.

F 2

After

* In several parts of his book Dr. Monro has proved logically, that my *conclusions* were *contradicted by premises*, and therefore from me they were unjustifiable: yet these very *conclusions* were found to be right, when he had taken a great deal of pains and drawn them from *proper premises*. He must surely wonder how I had hit upon such truths, without making any observations and experiments, and without any ideas of relation, connection, and inference. Call it a power, a gift, second sight, or what you please, it is something that has set me right several times. I shall give an instance of its influence. At the time my brother was working upon this preparation, I told Dr. Smith, (but in confidence as he remembers) that Dr. Monro would next undertake the *lymphatic glands*. This prediction was to be sure at that time against all human reason or probability; it was a *conclusion contradicted by premises*, and yet the very thing happened. See his note, pag. 40.

After giving this account of what was publicly done concerning the lymphatic glands, I shall make some remarks on this part of the Doctor's pamphlet. He quotes, (pag. 38.) that experiment of blowing or pouring mercury into the gland, and then says, "Dr. Hunter's assuming this experiment as his own, is certainly either the most undeniable proof of ignorance of what had been already done upon the subject, or the most palpable invasion of the property of our fore-fathers, if the expression can be allowed, that has ever perhaps appeared in print." How does the Professor prove this *strong assertion*? He *prates* about Nuck and Cowper, and finishes with the following flourish: "From which it was reasonable to conclude, that he, (Dr. Hunter) had not made any such experiment; and that he was but the echo of Nuck or Cowper." Now, do Nuck and Cowper say that they blowed or injected mercury by a pipe introduced into the lymphatic gland? No, neither of them says so; neither of them ever did it, or knew that it could be done, so as to fill the lymphatic vessels, and what they call the cells of the gland. They both injected the mercury by small pipes introduced into the lymphatic vessels. What a shameful misrepresentation is this in Dr. Monro, after such a direct and full assertion that "My taking the experiment as my own was either the most undeniable proof of ignorance of what had been done upon the subject, or the most palpable invasion of the property of our forefathers, that has ever perhaps appeared in print." That he should expose himself thus to the world! That his father should suffer him to do so!

In his next page, (viz. 39.) he appears with great advantage, (as he seems to think) in *proving* that this experiment is no evidence of the lymphatics being absorbents, and that therefore *Dr. Hunter had better omitted to mention it in proof of the general doctrine of the lymphatics being absorbents*. Dr. Hunter never thought it any proof of that doctrine, nor ever mentioned it as such. He mentioned it in his lectures, as a *new experiment* that might be useful, and as an easy method of exhibiting the lymphatics: and the authors of the Critical Review mentioned it without any direct application. A word to the wife, they say, is enough; but Dr. Monro would not take a hint. The experiment was mentioned, we may suppose, that he might not (among other things) take it to himself. Yet he has done so in the note of his 39th page; and it appears that he did so at Edinburgh in 1755, for he then told Dr. Black the experiment, and has got Dr. Black's certificate of having done so. Would he have done this, if it had been an experiment to be found in Nuck or Cowper?

Cowper? Did he then learn this experiment (among many other things) at Edinburgh from Dr. Hunter's pupils? Can we doubt, if we trace him a little upon the subject? It is plain that in the summer 1753, he did not know this experiment, or easy method of filling the lymphatics. "In summer, 1753," says he, pag. 22. "I first attempted to fill the lymphatic vessels with quicksilver, introduced by pipes put into openings made in some of their smaller branches. But not succeeding well in this manner, I then tried to inject them in the reverse way from the thoracic duct, in hopes that the quicksilver would pass their valves, as it frequently did those of the heart and large arteries:----but, after several experiments, I was convinced that this was impracticable. ----But, as I greatly wished to have some preparation of these vessels, I next endeavoured, in imitation of Nuck, Cowper, &c." (Reader attend!) "to fill them from the arteries----but with no other success.--- At last I thought of employing what the painters call size, (a thin glue) from which I flattered myself with great expectations----but was likewise disappointed in several trials which I made with it," &c. To make the story short; he at length filled them in a *testis*, from the artery by extravasation.

Well then, the Professor allows that in summer 1753, though *he greatly wished to have some preparations of the lymphatic vessels*, and made all the use he could of Nuck, Cowper, &c. yet he could not inject them either with air, quicksilver, or thin glue. He did not then know (and Nuck and Cowper could not tell him) *the easy method of exhibiting and injecting lymphatics by throwing air or mercury into the lymphatic glands*. So far is clear. Now let us see when he first discovered it: for after so many difficult and unsuccessful *methods*, surely he would be much pleased when he found out this *easy method*. But he does not venture to tell us *how* or *when* he found it out; would have had it pass quietly for his own; and when he finds it challenged by Dr. Hunter, gives it to Nuck or Cowper, as you shall see.

In the year 1755, he knew this experiment (from Dr. Hunter's pupils suppose) and procured Dr. Black's certificate in the following words (pag. 28.) "You told me at the same time, that an easy method of exhibiting the lymphatics is, to fill the cells of the conglobate glands with air, which passes freely into such lymphatics as rise from them, to take their course towards the lacteal sac." From which it is plain, that he was communicating it as a new experiment, and as his own; and it

was

was understood so by Dr. Black, as appears by his letter to me on that subject, in which he says: "So far as I can remember when he said that he could raise the lymphatics in this way, he told it me as a piece of anatomical news, or as a curious anatomical fact, which I imagined was a discovery of his own." In his 39th page, he says in a note: "Tearing the outer membrane of the conglobate glands, and breaking their substance and pouring in mercury, had been my common way of shewing the lacteals of the second order, or lymphatics going forwards from them, as Dr. Black observes in his letter." You see, he says it had been his *common way*. Now it was not his *common way* in the year 1753, as we have proved from himself; he does not venture to tell us how long this had been his *common way*, or how he found it out, but would have had it pass for his own observation (in his *common way*); and when he found that Dr. Hunter took it to himself, (or rather gave it to his brother) he tells us, with a good assurance, (his *common way*) that it was "either the most undeniable proof of ignorance of what was done upon the subject, or the most palpable invasion of the property of our fore-fathers, that has ever perhaps appeared in print;" then lays down his facts or proofs (in his *common way*) of a cock and a bull, and Cowper and Nuck, and concludes (in his *common way*) that *Dr. Hunter had not made any such experiment, and that he was but the echo of Nuck or Cowper.*

The next thing that occurs in Dr. Monro's pamphlet will require a whole chapter for its discussion.

C H A P. V.

OF ABSORPTION by VEINS.

DR. Monro has taken a great deal of pains to discredit me upon a variety of subjects, which he might have known I should not chuse to dispute with him. Upon these occasions he misrepresents the facts, puts what words he thinks proper in my mouth, gives out opinions as mine which he should have known to be not so; and then shews his parts and his reading in confuting them. He would have lost nothing had he disputed with more modesty, and learnt to bear what he thought a triumph with more moderation. We shall take his chapter, page 55, *Of absorption by the*

the branches of the red veins, as an example of this sort of dealing with me. Pray, reader, stop here a few minutes, and consider that chapter carefully: it consists but of a few pages.

He makes me assert that the red veins do not absorb, and then you see how he treats me, and triumphs. What foundation has he for making me the father of this hypothesis? He quotes the Critical Review. Look to the place, and you will find he refers to some notes written by the authors of the Critical Review; not to any work of mine. Now, suppose I had written these notes; yet as they appeared under another name, and as he could not prove, or certainly know them to be mine, surely he had no right to use my name with the freedom he has done. A gentleman would not have done so for my sake; a man of sense would not have done so for his own sake. But the truth is, that I knew nothing of those notes, *directly* or *indirectly*, till I read them in print: the authors of the Critical Review know this to be true, and give me leave to say so in the strongest manner I please*. To shew Dr. Monro in his true colours, I must go farther still, and tell the reader that he must have known this was not an opinion or hypothesis that I maintained. He attended all my lectures. I appeal to all the MS notes of my lectures, and to all my pupils. Let them recollect or look into the second lecture; they will find that I say of veins, *they begin 1. from arteries; 2. from surfaces or cavities as absorbents*: and when upon the mesenteric veins, they may remember I explained the common doctrine, and commonly produced the drawing of a little hydraulic machine of Dr. Lieberkuhn, by way of illustration; and that I went no farther in opposition to the common opinion, than to say that I was not satisfied, that I had doubts, that all my observations were against the common hypothesis, but that I did not take upon me to say it was not the truth.

Indeed, in both my courses of the winter 1759-60, I went so far as to say, I *believed* that the red veins did not absorb, and gave my reasons for thinking so: but this was long after Dr. Monro's publication.

In different parts of my lectures I used to treat of the transfusion and absorption of fluids in animal bodies in the following manner:

“ I have

* The Professor has been followed through so many mortifying situations, that it is high time to shew him some pity. The reader is therefore intreated to compassionate the distress which he must have been in, when he thought of quoting those notes in the Critical Review *as my words*, in order to make *his defence*; which he has done in six different places, viz. pag. 19, 37, 53, 56, 58, and 65.

“ I have often considered with myself how the interstitial fluid gets
 “ into the smaller and greater cavities of our bodies; how the water of
 “ an *anasarca*, for instance, gets into the cellular membrane. The com-
 “ mon opinion, I think, is, that there are every where exhalant arteries,
 “ which open and terminate on the superficies of such cavities, and throw
 “ out the watery fluid which they contain. But for my own part, I
 “ cannot help believing that it is entirely by transfusion through the
 “ coats or sides of the vessels. My reasons for thinking so are these:

“ First, so far as I can find, all the arguments of the latest and best
 “ anatomists, taken from injecting the arterial system in dead and living
 “ bodies, only prove that a thin fluid passes readily from the arteries into
 “ the interstices of parts. They do not prove the existence of exhaling
 “ branches, no more than they prove transfusion.

“ In the second place, the phenomena of injections, so far as I have
 “ been able to make observations, agree better with the notions of transfu-
 “ sion than with that of exhaling arteries. I have had great experience
 “ of injections, and I have made experiments with all sorts of fluids in-
 “ jected into the arteries and veins of dead bodies. I have always ob-
 “ served that subtile and penetrating fluids pass with ease from the ar-
 “ teries into the cavity of the intestine, and into the cellular membrane
 “ in any part of the body: such fluids are water, gum water, whites
 “ of eggs strained, glue, isinglass dissolved in water or spirits, any fluid
 “ oil, melted butter or axunge, &c. But when these fluids were co-
 “ loured with vermilion, I always observed that none of the vermilion
 “ passed out of the arterial system, but when there were manifest ap-
 “ pearances of extravasation and rupture of the vessels: I never observed
 “ vermilion pass into the cavity of an intestine from the mesenteric ar-
 “ teries, without seeing a hundred ruptures and extravasations in the *villi*
 “ of the gut. All this looks as if the fluid oozed through the coats,
 “ rather than was poured out by the branches of arteries.

“ In the third place, I have observed that the cellular membrane is not
 “ so immediately filled by injecting the arteries; it requires some time,
 “ and I have plainly seen, when I have let an injected part lie by a lit-
 “ tle while, that the cellular membrane became gradually more loaded
 “ as the arterial system became more empty; a strong argument, in my
 “ mind, that it got out of the arteries by transfusion.

“ In the fourth place, water, and even red blood soaks through all our
 “ vessels and membranes in dead bodies; as you may see by steeping the
 “ apex of a heart well washed, or the convolution of a piece of fresh
 “ in-

“ intestine in clear water: in both cases the water will become
“ bloody.

“ But still it is said that in all these cases the fluids pass by fine exhal-
“ ing vessels, though these vessels cannot be seen. To this I answer, that
“ if our interstitial fluid was of a strong marked colour, we should then
“ by dissection be able to observe whether it was poured out by small
“ arteries, or whether it soaked through the natural pores in the coats
“ of vessels. Now very fortunately for us in this dispute, there is one
“ such fluid in the body; it is the bile. Its colour is pretty deep, and
“ very different from any thing that lies near the gall bladder. No man
“ can have opened any number of bodies, without allowing that the gall
“ does pass in living bodies through all the coats of the gall bladder,
“ and pervades the substance of the neighbouring parts, not by
“ exhaling nor by inhaling vessels, but by manifest transudation or
“ soaking.

“ It might be asked, why the red blood does not transude through the
“ vessels in living bodies, for I think it certainly does not. In answer to
“ this it may be said, that our fibres and vessels have perhaps some de-
“ gree of tension and firmness in life, which they lose with life; and it
“ must be observed too, that in proportion as the blood putrifies it be-
“ comes thinner; whence we see, in opening a putrid body, all the cavi-
“ ties more or less filled with a bloody water, and all distinction of co-
“ lour in the muscles and cellular membrane quite lost. But what I
“ suppose to be the principal reason that red blood does not transude
“ through the vessels in living bodies, is its glutinous quality, its thick-
“ ness while it is equally mixed up with its coagulating part. That
“ part coagulates as certainly as the blood stagnates even in living bo-
“ dies; and when the universal stagnation happens in death, this part
“ of the blood collects itself into irregular *polypi* and coagulations all
“ over the body, and the rest of the blood is no longer the thick viscid
“ fluid it was before, but rather a bloody *serum*, that will ooze through
“ all the vessels and membranes.”

Such were my notions of the *source* of our interstitial fluid. With
regard to its *absorption*, I was of opinion that nature had provided a system
on purpose, viz. the lymphatics. I considered these vessels and the
lacteals as an appendage to the venal system, by which the stores were
brought in for supplying the circulation; and the glands and secretory
vessels all over the body, I considered as an appendage to the arterial
system,

system, by which the proper separations were made, and the redundancies thrown off.

My only doubt was, whether the veins did or did not absorb a certain quantity, especially in the intestines. From my own observations on injections, I should have concluded that they did not, and that there was no passage for liquors between an intestine and the mesenteric veins, otherwise than by transfusion. But authors of the best credit had given such arguments and experiments in favour of absorption by veins, that I dared not, even in my own mind, determine the question.

At this time my brother was deeply engaged in physiological enquiries, in making experiments on living animals, and in prosecuting comparative anatomy, with great accuracy and application. It is well known that I speak of him with moderation, when I say so. He took the subject of absorption into his consideration, and from all his observations, was inclined to believe, that in the human body there was one, and but one system of vessels for absorption: he knew so well that many things had been asserted by one person after another, which were not true, that so many mistakes had been made from inattention, so many errors introduced from other causes, that he could easily suppose the veins might not perhaps absorb, after all the demonstrations that had been given of the fact; and therefore was determined to see how far this point could be cleared up by plain experiments and observations. With that intention he made the following experiments, in my presence, and in presence of a number of gentlemen, who all of us assisted him, and made our own observations upon what passed before us. I shall quote the experiments from him, and can bear testimony to the fairness with which they were made, and with which they are here related.

A N I M A L F I R S T.

“ EXPERIMENT I. On the third of November 1758*,” says he,
 “ I opened the belly of a living dog. The intestines rushed out immediately. I exposed them fully; and we observed the lacteals filled
 “ with a white liquor at the upper part of the gut and mesentery;
 “ but in those which came from the *ileon* and *colon* the liquor was
 “ transparent.

“ I tied up the mesenteric artery and vein that was going to about half
 “ a foot

* In the presence of Doctors Clayton, Fordyce, and Michaelson, and Mess. Blount, Jones, Churchill, and Richardson.

“ a foot of intestine, and put a light ligature upon the upper part of
 “ the intestine, including a little of the mesentery, then emptied that
 “ part of the gut by squeezing it downwards, and put a similar ligature
 “ upon the lower part of the gut. In the next place, I made a small hole
 “ in the upper end of this part of the gut, and by a funnel poured in
 “ some warm milk, and confined it by making a third ligature upon the
 “ gut close to this hole. These ligatures prevented the circulation of
 “ blood in this part of the bowel. Lastly, I punctured the vein beyond
 “ the ligature that had been made upon the mesenteric vessels, and by
 “ gentle stroking with the end of the finger soon emptied it of its
 “ blood.

“ EXPER. II. I immediately after this made the same experiment,
 “ and in the same manner, on a part of the intestine lower down, where
 “ the lacteals were filled with a transparent liquor.

“ In the first experiment the lacteals continued to be filled with a
 “ milky or white fluid: in the second, the lacteals, which before con-
 “ tained only a transparent lymph, were presently filled with white milk.

“ In both these experiments, we could not observe that the least white
 “ fluid had got into the veins. After attending to these appearances a
 “ little while, I put all the bowels into the *abdomen* for some time, that
 “ the natural absorption might be assisted by the natural warmth; then
 “ took out and examined attentively the two parts of the gut and me-
 “ sentery upon which the experiments had been made: but the lacteals
 “ were still filled with milk, and there was not the least appearance of a
 “ white fluid in the veins: on the contrary, what little blood was in
 “ them, was just as thick, and as deep-coloured as in the other veins,
 “ and when squeezed out from them, coagulated as the blood of other
 “ veins.

“ EXPER. III. I tied up and filled another piece of the intestine with
 “ milk in the same manner, but did not make a ligature upon the me-
 “ senteric vessels, leaving a free circulation in the part. We looked very
 “ attentively at the colour of the blood in the veins of that part, both
 “ with our naked eyes and with glasses: we compared it with that in the
 “ artery, and in the neighbouring veins, but could not observe that it
 “ was lighter coloured, nor that it was milky, nor that there was any
 “ difference whatever.

“ EXPER. IV. Lastly, we took that part of the gut which was filled
 “ with milk in the first or second experiment, and squeezed and pressed
 “ it very gradually, in order to see whether any milk would by these

“ means pass into the empty mesenteric veins. This we did gradually
 “ with more and more force till the gut at last burst; but still there was
 “ not the least appearance of any thing milky in the veins.

A N I M A L S E C O N D.

“ EXPERIMENT I. November 13, 1758 *, I opened the *abdomen* of
 “ a living sheep, which had eat nothing for some days, and upon ex-
 “ posing the intestines and mesentery, we observed the lacteals were
 “ visible, but contained only a transparent watery fluid. I made a hole
 “ in the intestine near the stomach, and by a funnel poured in some
 “ thin starch, coloured with indigo, so as to fill several convolutions;
 “ then tied up the hole in the gut, and put all the bowels into the
 “ *abdomen* for some time. Upon taking them out after this, we observed
 “ all the lacteals of that part filled with a fluid of a fine blue colour.
 “ We thought at first, that the blood in the veins of this part was of
 “ a darker colour; but upon comparing it carefully with that in the
 “ other veins it was manifestly the same.

“ EXPER. II. I opened a vein upon this part of the mesentery, and
 “ caught a table spoon full of its blood. I set it by to congeal and
 “ separate into its *coagulum* and *serum*. On the next day, and the day
 “ after that, I examined the colour of the *serum*; but it had not the
 “ least blueish cast.

“ EXPER. III. I fixed an injecting pipe in an artery of the mesentery,
 “ where the intestine was filled with the blue starch, and tied up all
 “ communications both in the mesentery and intestine, (as in Animal
 “ First, Exper. I.) but left the corresponding vein free; then I injected
 “ warm milk by the artery, till it returned by the vein, and continued
 “ doing so till all the blood was washed away, and the vein returned a
 “ bright white milk. This was done with a view of seeing if the milk
 “ in the vein acquired any blueish cast; but there was no perceptible
 “ difference between the arterial and venal milk.

“ EXPER. IV. After this I opened the vein with a lancet, and dis-
 “ charged most of the milk, then put a ligature upon both the artery
 “ and vein, and waited some time to see if they would fill, but they did
 “ not, nor did the remaining contents of the vein acquire the least
 “ blueish cast. Then I opened the gut at this part, but we could not
 “ observe

* In the presence of Doctors Wren, Fordyce, and Michaelson; and Mess. Blount, Tickell, Churchill, Paterfon, and Skeette.

“ observe any appearance of the milk having got into the cavity of the
“ intestine.

“ EXPER. V. I filled another part of the intestine with milk. All
“ that we observed after doing this, was that the lacteals became
“ fuller, though not of a white colour, and the veins remained of the
“ same complexion.

EXPER. VI. I fixed a pipe into a vein of the mesentery, and injected
“ milk towards the intestine, to see if any would pass into the cavity of
“ the gut: but presently innumerable extravasations happened; so that
“ the experiment was fruitless.

“ EXPER. VII. I fixed a pipe into an artery, and tied up the vein, and
“ all the communications, then injected milk for some time into the
“ artery till the vein became quite turgid and tight; this was continued
“ for some little time, and with as much force as we thought the vessels
“ would bear without bursting: then we opened the intestine at that part,
“ and there was no appearance of milk in its cavity.

EXPER. VIII. I took a piece of the intestine that was quite empty
“ and clean, and filled it with warm water. The returning blood in
“ the vein of this part appeared not at all diluted or thinner than in
“ the other veins. Then I tied up the artery, and all the communica-
“ tions; and attended to the state of the vein for some time: it did not
“ grow more turgid, nor did its blood become more watery, nor was
“ there any appearance whatever of the water's having got into the
“ veins.

“ The animal was quite alive all the time of our making these experiments
“ and observations, which lasted from one o'clock till half an hour after
“ three. I chose a sheep rather than a dog, both because the animal was
“ much larger, and therefore its mesenteric vessels were fitter for being
“ easily injected; and besides, because it is much more patient and
“ quiet. These advantages we were all sensible of when we made the
“ experiments.

A N I M A L T H I R D.

“ June 22d, 1759. We repeated most of these experiments on
“ another sheep, to see if the effect would be the same: but in this
“ animal the viscera were diseased, inflamed and thickened in most parts,
“ so that the experiments were much less successful, less satisfactory, and
“ conclusive. After injecting milk into the mesenteric artery for some
“ time, and allowing it to return by the vein, we opened that part of
“ the

“ the intestine, which had been previously emptied, and found in it a
 “ watery fluid of a whitish cast, as if a few drops of milk had been
 “ mixed with it.

A N I M A L F O U R T H.

“ In July, 1759 *, I repeated most of the experiments related in
 “ article *Animal Second*, upon another sheep. The effect of all of them
 “ was so nearly the same that I need not be particular.

“ I shall only observe, that when the intestine was filled with starch-
 “ water and indigo, and milk injected by the artery till the vein was
 “ washed clean of blood, and a ligature put upon the artery and vein, so
 “ as to leave them about half full of pure white milk, after waiting
 “ more than half an hour we could not observe that the vein was in the
 “ least more filled or turgid, nor had the milk in the veins acquired the
 “ least of a blueish cast, nor even in the smallest veins upon the gut itself,
 “ where we should suppose the absorbed liquor must have been appa-
 “ rent, if any had been taken up by the veins from the cavity of the
 “ intestine.

“ After the animal was dead, I blowed into a mesenteric vein, and
 “ the air found a passage into the cavity of the gut, though in making the
 “ experiment when the animal was alive, I could not force the milk by
 “ injection from the vein into the gut.

A N I M A L F I F T H.

“ If any animal could be supposed a fitter subject for such experiments
 “ than a sheep, it would be an afs. He is not so large, nor so strong,
 “ but that he may be managed; he is patient in the greatest degree; his
 “ mesentery and vessels being larger, it is so much more easy to fix
 “ injecting pipes, make ligatures, &c.: and, what is a very great ad-
 “ vantage in making such experiments, his mesentery is very thin, with-
 “ out fat, so that the vessels are conspicuous and distinct. Hence it is
 “ easy to separate the artery from the vein, to fix pipes, to tie up anasto-
 “ mosing vessels by a needle, &c.

“ Therefore I got an afs, and on the 24th of August, 1759 †, put him
 “ upon

* In presence of Doctors Macaulay, Ramsey, and Michaelson; and Mess. Edwards and Tomlinson.

† In presence of Doctors Macaulay and Michaelson; and of Mess. Edwards, White, and Gee.

“ upon his back in an open garden, and tied him fast to four stakes driven
“ into the ground, then opened his *abdomen*, &c.

“ EXPERIMENT I. I poured a solution of musk in warm water into
“ a piece of the intestine, and confined it there by two ligatures. In
“ doing this the animal struggled, and a little of the liquor was spilt
“ upon the outside of the intestine and mesentery.

“ After waiting a little while, I opened with a lancet some lacteals of
“ this part, which were full of a watery fluid, and caught a little of
“ their contents in a small spoon. It smelled strongly of the musk; and
“ though it could hardly be doubted that the musk had been taken up
“ from the intestine by absorption; yet as some of the musk-solution had
“ been spilt upon the external surface of the parts, and as it was im-
“ possible to collect the lymph from the lacteals without resting the edge
“ of the spoon upon the mesentery, the smell of the spoon might be
“ owing to that circumstance.

“ After this I wiped a vein upon the mesentery very clean, and opened
“ it with a lancet: a gentleman who had kept out of the way of the
“ musk, came immediately with a clean spoon, and filled it from
“ the stream of blood without touching any part of the animal, and car-
“ ried it directly off; but it had not the least smell of musk.

“ EXPER. II. We poured some starch water, made very blue with
“ indigo, into a part of the gut in the same manner as in some of the
“ former experiments; tied the vein and artery of this part; then punc-
“ tured the vein close to the ligature, and pressed out almost all the
“ blood; then tied up the empty vein, and put all into the cavity of the
“ belly for a quarter of an hour. After that, we examined the part,
“ found the lymphatics very turgid, as the fluid could not pass through
“ them towards the thoracic duct, on account of the ligatures made upon
“ the mesenteric vessels: but we found the veins of this part empty, ex-
“ cept indeed that a little blood had got into them from the neighbouring
“ vessels, which, from the appearance, had evidently passed the ligatures
“ tied round the ends of the gut; a circumstance which it is very dif-
“ ficult to obviate.

“ EXPER. III. I next repeated the *Third Experiment of Animal Second*
“ exactly in the same manner, and precisely with the same effect.

“ EXPER. IV. Then I repeated the *Fourth Experiment of Animal Se-*
“ *cond*; and the effect was still the same.

“ N. B. It may not be amiss to observe, that the lacteals continued to
“ absorb the blueish liquor all this time; even at the part upon which
“ this

“ this *Fourth Experiment* was made, where the nerves must necessarily
 “ have been tied up with the artery.

“ EXPER. V. I squeezed a piece of the intestine so as to empty it as
 “ entirely as well might be, then tied up all the lateral communications
 “ of the vessels, and injected warm milk into the mesenteric vein till it
 “ returned by the artery, and continued this operation for some time af-
 “ ter all the blood was washed out. Then I opened that part of the in-
 “ testine through its whole length, and found it quite empty.

“ I made this experiment again upon another part of the intestine, in
 “ the same manner, and exactly with the same success.”

Here is a new doctrine proposed in physiology, viz. *that the red veins do not absorb in the human body*. The fair enquirer after truth will be convinced, by the observations which occurred to me, that the common opinion is supported by some proofs that are at least doubtful or equivocal, and that the other opinion is not without plausibility; and he must allow that my brother's experiments render it highly probable.

But we may presume from what Dr. Monro has argued upon the doctrine of the lymphatics, that he will say I had no right to set up this hypothesis, except *I could refute, or otherwise explain* the experiments and arguments of the best and latest physiologists in support of the common opinion. I will therefore shew how little Dr. Monro has been able (with all the assistance which he may have had) to establish the old opinion, or to disprove the other.

The first thing that he urges (pag. 56) is an inconsistency between two expressions which he is pleased to call mine. My answer is, that I wrote *one of them*, and the authors of the Critical Review wrote *the other*. Yet there appears to me no inconsistency. For, if we suppose that no small veins do begin as absorbents from the cavity of the intestines, &c. may not veins, as instruments of the circulation, begin from the cells of the *penis*? And though the chyle or lymph could not well pass through the absorbent system without valves and lateral pressure, may not blood return easily by the veins, where there are cells between the terminations of the artery and beginnings of the vein? With regard to the motion of the contained fluid, these cells are to be considered, as enlargements of the canal, as something of an intermediate nature between an aneurysm and a *varix*: so that the blood is pressed from the cell into the vein, with the same force with which it was thrown from the artery into the cell. Hence it is that the *penis* remains in a state of systole, so long as the resistance to the venal stream

stream does not overcome the natural contraction of the part; but as soon as it does, the extension or *diastole* of the part necessarily ensues, and must continue as long as that resistance is continued. I shall prove this by an experiment which my brother made upon a living dog, and which I shall give in his own words. "In April 1760," says he, "in presence of Mess. Blount, &c. I laid bare the *penis* of a dog almost thro' its whole length, traced the two veins that come from the *glans*, which in this animal makes the largest part of the *penis*, and separated them from the arteries by dissection, that I might be able to compress them at pleasure, without affecting the arteries. Then I compressed the two veins, and found that the *glans* and large bulb became full and extended. I then irritated the veins, in order to see if there was any power of contraction in them, which might occasionally stop the return of the blood; but could not observe any such appearance. Then I pricked the *erectores penis*, upon which they contracted; but their contraction had no sensible effect upon the veins or upon the *penis*."

Dr. Monro's next argument (page 57) is such as can have no weight against the new hypothesis. It is a long chain of premises, each of which may, or may not be true; and at last there is a *conclusion* which is very agreeable to our hypothesis, and a presumption about a point that is not in question. It runs thus: *Lacteals have not been certainly observed in oviparous animals*, and yet, God knows, they may have them.---Dr. Monro is convinced they have not lymphatic vessels, and we all know he may be mistaken;-----therefore in them the red veins absorb, or something else does it.-----But as we cannot observe that in them the veins differ in structure from the sanguineous veins in man, and yet it must be granted they may differ, and nobody has yet proved whether they do or not. Hence, (true or false, right or wrong, according to the professor's logic) it is not necessary that absorbent vessels should have the valvular structure of the lymphatics. No surely: capillary vessels, for instance, will absorb without valves; but vessels like the lymphatics or lacteals, that are not only to absorb, but to convey fluids a considerable way unassisted by gravitation, require valves, or something that shall have the same effect, such as the muscular peristaltic motion of the great tractory vessel that goes from the mouth to the anus. And it is also to be presumed, says our author, that the structure of the branches of the red veins in man is such as renders them capable of absorbing. We, on the other hand, presume that if they were capable of doing it, they would actually do it; which does not ap-

pear to be the case; and we *presume* if their small branches do not open into the cavities, that they may not be so capable of doing it.

Our author's next argument (page 57 and 58.) is drawn from the allowed absorption of the *placenta*.-----“ Yet,” says he, “ whatever diligence I have employed here in search of lymphatic vessels, has proved as fruitless as the labour of others had done. It remains then to Dr. Hunter to prove the existence of valvular lymphatic vessels in the *placenta*: or, by allowing that the branches of the umbilical, that is, of the red veins do absorb, to retract such *crude notions*; which betray a want of due reflection, even on a subject about which the Doctor is daily occupied.” Surely, notwithstanding all the Professor's *diligence*, there may be ten thousand short lymphatics in the *placenta*, terminating in the branches of the vein. Supposing there were, must the Professor have seen them? Perhaps not with all his diligence.

Now let us consider his *more direct proofs*, (pag. 58.) “ Fluids,” says he, “ injected from the trunks into the branches of the veins sweat out upon the surface of the skin, and into the different cavities of the body *; which evidently shows that many of their branches begin from these; and hence must be inhalent.” Here is an assertion, and an inference. The assertion consists of two parts, which must be considered separately. *Fluids injected into the veins sweat out upon the surface of the skin*. Dr. Haller says no such thing in the place referred to †; and Kaau Boerhaave says nothing like it in his 3d, 4th, and 5th chapters, where

“ * See the elegant treatise of Kaau Boerhaave *De Perspiratione*: Or, *Element. Physiolog.* of the illustrious Dr. Haller, Lib. ii. S. 2. § 22, 23, 24.”

† On the contrary, all he says there of the cutaneous veins is this, *Inhalationem, sive humoris per cutem resorptionem, proprio loco ostendemus*, and then in a note desires us in the mean time to consult his Commentary on Boerhaave, Tom. III. n. 416, where no such experiment is mentioned; but in the following, n. 417, after speaking of the arteries which may be demonstrated by injections to terminate on the surface of the skin, he says: *De venis experimentum non novi, ob valvulas, sed analogia demonstrat, esse quæ respondeant arteriis*. So that to prove an anatomical experiment, our Professor quotes Baron Haller, who says he knows no such experiment. Please to observe, I speak here of an anatomical *fact*, not of an *opinion*. It is Baron Haller's, and every body's *opinion* that inhaling veins begin from the surface of the body; and accordingly in another part of his writings, *Prim. Lin.* § 444, he says: *Demonstrat has venas injectio anatomica, quæ per venas perinde, ut per arterias exsudat, si aquosa & tenuis fuerit*: and what that *exsudatio per arterias* is, he tells us, § 435, &c. *Guttulæ sub epidermide effusæ (quæ) hanc in vesicas elevant*. But if Dr. Monro had found out these two passages in Haller, they would not have answered his purpose, except he could have proved that *blistering* was the same thing as *sweating*.

where he professedly treats of the cutaneous discharges; and therefore we presume he no where says so. We will venture to say the assertion is not true, and that the Professor will find it to be without any foundation, if ever he shall make a fair trial. This assertion then of the Professor cannot be allowed to have any weight: nor can we allow much to what he says in the following page, (59) *In such trials as I have made, fluids get more readily into the cavity of the guts from the veins than from the arteries; and this ready outlet into the cavity of the guts, &c.* The Professor does not seem to have practised injections with sufficient dexterity and circumspection; without which they will often mislead us egregiously. In *such trials* as he made, injections passed more readily, &c. Why? In plain English, because they sooner burst, which gives a *ready outlet* enough. By *such trials* he may find a *ready outlet* from the right auricle of the heart into the cavity of the *pericardium*, and from the larger vessels of the brain into its ventricles, and several others which no anatomist ever yet dreamt of. That our author's *trials* were frequently *such*, appears too from what he said in page 23, viz. *In injecting the arteries indeed, for example the mesenteric, I had often observed that the injected matter passed more readily by the lateral branches into the cavity of the guts, than into the corresponding veins.* From all which, those who are much conversant in injections, must see how he was going on.

The passage of fluids from one vessel or cavity into another, must not always be taken as a proof that they have natural communications. Experiments of this kind made with water, air, or quicksilver, are particularly unsatisfactory. Water transudes readily through, perhaps, all parts of the human body, except the cuticle; and air and mercury are very apt to produce rupture in the vessels, long before they have run into such minute branches as are easily filled with glue, &c. even when coloured with the powder of vermilion. Hence we see that in the dead body, air blown into the *trachæa* not only readily passes into the pulmonary vein, but finds a *ready outlet* at a thousand places through the external coat of the lungs. Any of the above-mentioned fluids readily pass from the emulgent vein into the ureter; and mercurial injections into the arteries and veins of most parts of the body, produce rupture and extravasation long before such minute branches, as we can easily inject by other means, are filled. This is not owing to the want of fluidity in the mercury, but probably to the strong attraction among its parts, which resists its being drawn out into fine threads: and hence it runs so well

through the excretory system of the *testis*, where the coats of the tubes are so much thicker and stronger *.

From what has been now said, and from what was advanced upon the subject of transfusion in the former part of this chapter, and from my brother's experiments, the reader must judge of the experiments which
Dr.

* Anatomical injections have certainly improved physiology, and yet they have sometimes led anatomists into great blunders. From the simplicity of the art of injecting, any man who can fix a pipe and melt wax, may set about experiments of that kind; and may draw conclusions about the delicate and complex operations in the living body, from the event of a blundering experiment made upon a dead one. If his wax breaks through the vessels at any part, it will be a *direct proof* that there was a natural passage; and if by escaping at this *ready outlet*, it does not run into the smaller vessels, it proves clearly that no such small vessels exist, and that those anatomists who have seen them, and who have injected them, must have mistaken one thing for another. Our young Professor is one of those who make bold injections and bold conclusions. His father has the same pretensions to injections, and has the same knack of going on in the argument, whether the wax stops or runs. I shall give a specimen. He twice, in a bitch, injected the uterine vessels with mercury, and found that he had not thereby filled any vessels in the *placenta* (a); thence he concludes that in that animal the uterine vessels do not ramify through the *placenta*: and yet I find it as easy to prove by injections that they do, as that the mesenteric vessels go into the coats of the intestines. Having in this way established his supposition, that the uterine vessels do not in the human subject pass into the *placenta* and *chorion*, he afterwards found it would be necessary in support of his opinion to take off Noortwyk's authority (b), and therefore tells us that Noortwyk must have made a *mistake*, and is persuaded he will alter his opinion, *when the mistake is pointed out to him*; and, what is still better, concludes that Noortwyk *has afforded him a very pretty proof of there being no anastomosis between the vessels of the uterus and secundines*. Professor Monroe would only have you suppose that Dr. Noortwyk had mistaken the *uterus* for the *placenta* and membranes; and, in order to give his argument some plausibility, he has *egregiously misrepresented* the Doctor by substituting *one word for another* of a quite different meaning, in two of his quotations, and in a third has *coined a whole sentence* for him; &c. &c. Now the fact is, that Noortwyk was not mistaken, and is, we may presume, of the same opinion still, as it is impossible he should not believe his own eyes. I have seen his preparation, and can vouch for the truth of the greatest part of what he advances, not only from having seen that preparation, and from having conversed with him upon the subject, but *from my own experiments and observations on the human uterus, and those often repeated*.

I went into this digression to shew a little of the genius of our two Professors; to shew their easy and dictatorial way of writing under a weight of distressing circumstances. It was for such a spirit in Dr. Monroe's whole pamphlet that I despised it; and it was such a dictatorial stile, and such a parade of appealing boldly to facts, that made my friends think that the performance might impose upon some unexperienced readers.

(a) Med. Essays (Edit. 1752.) Vol. II. pag. 130.

(b) Ibid. pag. 118.

Dr. Monro has quoted as authorities against the doctrine which I have been proving and defending. I grant that he has produced some of the greatest *names* in anatomy; but it is an argument that may be brought against every new doctrine, and will have little weight when put into the scale against facts.

C H A P. VI.

Of the Vessels of CARTILAGES, and of the Ducts of the LACHRYMAL GLAND.

DR. Monro (pag. 61.) has given me an opportunity, which I embrace with pleasure, of correcting in print one of my own errors in anatomy. When I was a young man, and a very young anatomist, I wrote a paper concerning Cartilages, which was published in the *Philosoph. Trans.* Vol. 42. In that little essay I advanced that the vessels of the articulating cartilages pass, secure from friction and pressure, between the gristle and the bone. This appearance I saw distinctly in several injected joints: but afterwards I found out that the vessels, which I had seen, did not belong to the superficial cartilaginous crust which remains in the adult, but to what lies immediately under it, in growing animals, before the ossification is complete. My pupils will bear me witness, that I confessed and corrected this error, in every course of my lectures; and so far as I know, it was never taken notice of except by myself, till Professor Monro jun. did it that honour, after he had attended my lectures.

I grow so tired of my author, that I will only add one criticism more. It shall be upon his last Chapter, viz. *Of the Lachrymal Gland and its Ducts*: a new subject, which should not have been brought into the dispute: but by his manner of treating it, we see how little he is disposed to do me justice, and how readily he will expose himself to disagreeable suspicions, for the sake of being thought a discoverer in anatomy. He tells us that *several of the most celebrated anatomists, particularly Morgagni, Vater, Haller, and Zinn, had exhausted their patience to no purpose in quest of such ducts in the human subject, &c.* Then he comes to his discovery, viz. In the summer in 1753, (the ever memorable year still) he sought after these ducts, discovered their orifices, and introduced bristles into them; and shewed this to his father, who always *mentioned it* in his lectures since that time, &c. &c.

By

By this time, I fancy none of my readers will be surpris'd when I tell them that I had done the same thing, and that I had taught it in every course of lectures ever since the year 1747. If it were necessary to prove a thing so notorious, I could produce many testimonies, particularly that of Mr. Symons of Exeter, who attended my first course of lectures, and of Dr. Smith of Oxford, who both remember that they saw me introduce bristles into the ducts * of that gland in the human subject, and that they assisted me in doing it.

Is it not very singular that the young student should have made so many discoveries in the year 1753 at Edinburgh, and that all of them should have been published before that time at my lectures in London? Is it not pretty plain how he came by them, when we are informed, that every winter there were some students at Edinburgh who had attended my lectures; and about this very time some of these gentlemen belonged to a society, of which he was a member, instituted by the students for talking and disputing upon medical subjects? Does it not agree with, and support this insinuation, that he published such things as his own, after he had heard them from my mouth before a numerous audience, without doing me the justice of putting my name into a marginal note? It would have been but justice, if he had really and truly made the discoveries himself; and if he had, would not he have done so?

After all that has been said and proved, the reader will surely be entertained with our Professor's conclusion †, which I think a specimen of

* I make no doubt but that all, or much the greatest part of the tears come from the *glandula lachrymalis*. That they do, and that this gland, small as it is, can secrete a considerable quantity of liquor in a small space of time, will appear from the following experiment which my brother exhibited to my satisfaction. He inverted the upper eye-lid in a living sheep, and we could distinctly see the tears or water gushing in considerable quantity from the orifices of the ducts of the gland. Every time the part was wiped dry, the gush was very manifest, and when it was confined for a moment by pressure, the succeeding gush of water was in greater quantity, and more violent.

“ † Upon the whole, I must conclude, that, altho' in my dissertation on the lymphatics
 “ I have referred to almost every author on the subject, the public will allow I have been
 “ guilty of no omission, in not taking any notice of Dr. Hunter; as I have shown that he
 “ did not mention any fact which was not to be met with in common books; that his con-
 “ clusion from these facts was altogether improper; and that he farther denied the office of
 “ absorption to the branches of the red veins, contrary to reason and experiment.

“ I am hopeful too, that the Doctor himself, upon considering this, will not only excuse
 “ my not having mentioned him, where it could have been so little to his praise; but
 “ that he will also think himself obliged to me, that, so far from having industriously sought
 “ the occasion of fixing dishonour upon him, I even shunned it when it offered.

“ But

of the most easy behaviour under such awkward circumstances, that has ever been exhibited to the public.

To the best of my knowledge, I have stated the facts with the most perfect regard to truth. Were I, in my turn, to draw conclusions, I might perhaps be wrong. I will therefore leave that to others; and leave Professor Monro jun. to enjoy himself, and his discoveries; and to say and to print whatever he may think proper upon this dispute without molestation. He has established a character for ever with me, and I am resolved to take no farther notice of him.

C H A P.

“ But even upon the supposition that these observations used by Dr. Hunter had not been borrowed, and that his conclusion from them had been well founded; still there was no reason whatever for me to make the least mention of the Doctor, as I learned nothing from him; for these and many other observations, with various experiments, were remarked and fully explained, and the conclusion, that the lymphatics were a system of absorbents, was drawn from them in my treatise (a), previous to my acquaintance with him, or knowledge of his arguments.

“ Were I, before quitting the subject, in return for the profusion of the Doctor's good wishes for my sake, to offer him my best wish, it would be, that he had not attacked me at all; for, by that means, he has forced me, contrary at least to my intention, if not to my inclination, to bring to light many circumstances necessary for my own defence, from which Truth would not allow me to draw conclusions greatly to his honour: and for which therefore he has himself only to reproach.

“ If, however, the Doctor shall still persevere to alledge, that his cause is not so desperate as I have represented it, it is to be expected he will endeavour to make this appear in a plain way, by facts well vouched and conclusions fairly deduced from them.

“ For if, instead of these, he shall answer truth by exclaiming against me for telling it, because it happens to gall him; shall wrangle about words and expressions; shall affect not to comprehend, what the rest of the world may think but too plain; shall again insult the patience of the public by making presumptions upon presumptions, confessing, at the same time, that there is no certain knowledge in the case (b); in short, shall answer facts by suppositions; arguments and plain conclusions by evasion and perplexity, and an attempt at a sort of wit, which, especially in an affair of this nature, must ever recoil upon him that uses it: the discerning part of the readers, I presume, will allow, that I do the Doctor no injustice in concluding, that he gives up his cause and silently avows his conviction; and that he is labouring to raise dust, in order to screen himself and get off, like what story tells us of some of the combatants of old, who, when worsted, escaped in a cloud.”

“ (a) See the letters of Drs. Black and Reimarus, &c.”

“ (b) As in Critical Review, page 531.”

C H A P. VII.

An Examination of what Professor Monro sen. published as a Defence of his Son.

HAVING taken my leave of the son, I find myself under the disagreeable necessity of settling some matters with the father, my old master in anatomy. He took an early part in the dispute, as an anonymous author*; and if he had pleaded his son's cause with moderation and candour, he would have shewn a dignity of mind worthy of his years and of his rank in the profession. But this was so far from being the case, that I have the mortification to find him led away by his passions to misrepresent facts, and descend to quibbling, in support of his arguments and purposes; and this not only in that first attack, when he was less informed and his resentment therefore keener, but on many subsequent occasions, as I have been credibly informed, and particularly at his public lectures, when I should think he must have known a little more of the grounds of the dispute. It is true, that after what I have said in the former part of this work, it may seem unnecessary to take notice of what the senior Professor has advanced in that anonymous performance; yet there being some indirect accusations in it, which I could not so well bring into the former part, I hope the candid reader will indulge me with a little more of his time and patience.

In the first seven paragraphs our author gives an historical account of the young Professor's discoveries and publications, to shew that he could not have learned any thing concerning the lymphatics from me. I have already given the history of the dispute, with *proofs* of all the material points; and Professor Monro sen. has forfeited all reputation as an historian, by telling us, in the end of the first paragraph, that he *knows the facts relating to the present dispute, and sends a fair state of them*; and then by assuring us in the sixth paragraph, that *Dr. Monro went to London in absolute ignorance of Dr. Hunter's having any particular opinion concerning lymphatics;-----attended his lectures, and was surprised when he heard Dr. Hunter teach the doctrine of lymphatics being absorbents.* Dr. Black's letter shews what credit is due to these assertions.

I shall

* Critical Review for November, 1757. See Appendix, No. II.

I shall therefore pass over this part, and indeed all that is contained in the letter, except the three last paragraphs, which I shall consider, under the three following articles.

1. Why I do not anticipate others by an early publication.
2. How far the right belongs to him, who first puts a discovery into print.
3. How far it is true that I have *perpetual disputes with Pott, Haller, Albinus, Monro, or perhaps twenty more, that will prove very troublesome, and will at last redound very little to my honour.*

When Dr. Monro asks why I do not anticipate others by an early publication, I own I do not perhaps understand the meaning of the question. If he means to advise me to be silent on any thing, which I may think a discovery or improvement, till I have put it into print, lest it should be stolen from me, and occasion a troublesome dispute; I am surely much obliged to him for such affectionate attention to my peace of mind and reputation. But if he means that I should print my observations before I am prepared; I cannot take his advice.

He that is in a hurry to publish discoveries, will often have occasion to repent of his haste. Reflection, and more favourable opportunities of making inquiries, will at length bring us back to truth, if we have been misled; and will confirm and improve our inventions, if they be right. Had I printed my thoughts about the lymphatics in the first years of my reading lectures, they would have been more imperfect, because the proof drawn from poisonous absorption had not then occurred to me; and I am not so partial to the doctrine at this day, as not to see that something still remains to be done. The argument drawn from extravasated fluids passing readily into the lymphatics in dead bodies, is not as yet conclusive, though Dr. Monro has built so much upon it; for in this case there may be ruptures in the lymphatics, and the fluids may get into them by such means, rather than by their absorbing orifices. And this possibility will come nearer to probability, when we reflect, that the lacteals cannot, by any means now known, be injected or filled from the cavity of the intestine in the dead body: and if the lymphatics be similar in other respects to the lacteals, we ought to presume they are so likewise at their beginnings or absorbing orifices.

But the great thing still wanted upon this subject is to prove, by experiments made on living animals, that the lymphatics *do actually and certainly absorb*. We must throw milk, or other coloured fluids into the chest, *abdomen*, and into the cellular membrane of the limbs in living

animals ; and open them after a proper time, in order to see if the lymphatics be actually filled by these fluids. My brother and I have already made several experiments of this kind, as occasion offered : but the appearances have not as yet been clear and sufficiently satisfactory. It will likewise be necessary to watch morbid appearances, particularly in glandular diseases, dropsies, *emphysemata*, and in contusions with extravasated blood. Accurate observations and dissections of that kind would probably throw great light upon the subject. By way of illustration I shall give a case, which will confirm our theory of the lymphatics, and at the same time shew that it may be useful in practice.

In the spring 1759, a gentleman about 40 years of age, who had a very good constitution, and who lived temperately, consulted me about an inguinal tumour. It was manifestly a swelling of one or more of the glands, nearly as big as a pullet's egg, and almost perfectly indolent. It began without any known cause, and increased gradually. Though there was no probability, yet as there was a possibility of some insensible venereal absorption, I advised him to be put into a gentle mercurial course, and to wait the event. The swelling became larger every day, was hardly at all painful, and at length had a plain fluctuation. He was so much afraid of the lancet and caustic, that my brother, who was his surgeon, left it to burst of itself. It did so, and the discharge, which was extraordinary great, was principally a thin watery fluid. From the indolence of the tumour, and from the immense discharge for some days, we suspected it might come from the loins, or some other internal part ; but upon the most accurate examination which we could make, this did not appear to be the case. His health was otherwise perfectly good ; the fore healed slowly and gradually, without shewing the least mark of a venereal taint ; and the discharge, which was always watery, was in such a quantity, that it wetted through every thing he could cover it with, in a very little time, even when the wound was to appearance almost healed. Then the discharge became less by degrees, and at last stopped suddenly. The wound closed at the same time, and continued well ever after. But when the water began to flow in less quantity, the limb began to swell ; and as the stream from the wound diminished, the bulk of the limb increased. The swelling of the leg was anasarous, and it was general, and attended with no pain or inflammation, but with a considerable degree of weakness, weight, numbness, and unsuitness for motion. His general health was perfectly good, and the other leg quite sound. The nature of the case seemed to be very clear, upon the supposition of lymphatics being absor-

abforbents : thus, whatever had been the cause, the glands had been obstructed, and, in consequence of the suppuration, the principal lymphatics of the limb were so affected, that they could not transmit their contents to the thoracic duct : so that while the wound remained open, the absorption went on, and the lymph was discharged that way, as the *saliva* sometimes is by a wound in the cheek ; but as that vent ceased, the absorption was checked, and the whole leg necessarily swelled from the accumulation of extravasated lymph. In this way I explained the case as well as I could to the patient himself ; and told him farther, that as the wound had closed of itself, I could not doubt but that some passage still remained open for the lymph, which in time would become more and more free : and that I apprehended nothing more was to be done but to use a good deal of friction, especially upwards ; and when he was in bed, to keep his leg higher than the rest of his body. I observed to him likewise that he might try purging, sweating, diuretics, blisters, fomentations, &c. but that in my opinion he was principally to trust to time and patience. However, I advised him to have a consultation. To make the story short, Dr. Taylor was consulted, and confirmed my opinion ; and the event was likewise agreeable to it : for in about six weeks the leg was gradually restored to its full health and vigour.

I have often observed in women, who do not give suck, and in nurses, after they leave off suckling, that the axillary glands become painful, swell, and sometimes suppurate. Is not this owing to the acrimony which the milk has acquired by long stagnation in the breast, and affecting the gland through which it must pass in absorption ? I have observed too that they are at the same time liable to little fevers of the intermitting kind, but very irregular in their return, which come on with a *rigor*, and go off with sweat. Are not such fevers raised by the absorption of acrid milk ?

In December 1760, a male subject was brought to me, which, from the appearance of the stump, I supposed to have died two or perhaps three weeks after an amputation below the knee. The flesh of the stump seemed to have been in a bad condition, being putrid, and separating from the bone. The lymphatic glands of that side, at the upper part of the thigh, the groin, and upon the iliac vessels within the *abdomen*, were much swelled : and two of them, when cut into, were found compleatly suppurated : yet in no other part of the body, (which was injected and dissected for a demonstration of the blood vessels at my lectures) could we observe the least disorder in the lymphatic system. Was not this disorder

of these glands owing to the putrid absorption from the stump? And perhaps, if we had known the circumstances of the case, we should have had reason to believe that this absorption raised and fed the fever which carried off the patient.

Discoveries and improvements in the arts are not commonly brought to any tolerable degree of perfection in a little time; especially when they fall to the share of men who are much employed about other things, and when they require opportunities that seldom happen*; and with the best opportunities, and when managed by the ablest men, it has been thought better to throw them out first in a more private way, in order to take the opinion of other people, and hear the reasoning of those whose prejudices may ballance the partiality of an author. The GREAT HARVEY proceeded upon this cautious plan in the discovery of the

* For example, I have for many years taught the following new doctrine about luxations, viz. That when a dislocation is produced by violence in an healthy state of the joint, the capsular ligament is always lacerated, not simply stretched. I proved it to be highly probable from the anatomy of the joints, and from experiments made upon dead bodies; and shewed that the difficulty of reduction, in some cases, does not depend on the imaginary contraction of muscles, nor the impossibility in others, on the imaginary inspissation of the *synovia*; but that in a simple dislocation, the facility or difficulty of reduction may probably arise from the nature of the laceration; and that the impossibility of reducing an old dislocation is owing to the union of all the lacerated with the neighbouring parts. In my lectures, I always signified a desire of seeing such cases. Mr. Gataker was kind enough, some years ago, to gratify my curiosity upon one such occasion; and Mr. Thomson lately did me that favour, in a case that proved, in a most satisfactory manner, every circumstance which I had advanced; at least as far as one case could prove any general doctrine. Surely men of sense must think that it was right to give my thoughts and observations upon this subject at my lectures. It was doing justice to my pupils to give them every idea, however imperfect, that might be useful to them in their profession. Accordingly Mr. White of Manchester, soon after he had attended my lectures, applied this doctrine to practice in treating dislocations; and with great success, as appears by his paper in the second volume of Medical Observations, published at London; and in the letter which accompanied that paper, he did me all the justice that I could expect.

On the other hand, whatever may be Dr. Monro's opinion, I cannot but think it was right to suspend being an author upon this subject, till I had actually dissected some cases of dislocations, and could prove the doctrine in a more unexceptionable manner. At length, this doctrine has been published with advantage in the same volume by Mr. Thomson, who had too much sense and too much candour to suppress my name. But still it will be necessary to examine a number of cases, before it can be known with certainty whether the doctrine be in general well or ill-founded; and probably it will require the examination of a great variety of cases to explain the various circumstances, which may occasion particular exceptions to the more general rule.

the circulation*. When Professor Monro recommends example, does he think there is any one more respectable?

Another thing that kept me from writing upon the lymphatics, and upon some other subjects too, was the want of leisure to examine what had been already said of them by authors of reputation. If a man writes freely upon any subject, without knowing what has been said by others, he risks being made the object of ridicule or censure. I have several times met with my own observations in books, after having long believed them peculiar to myself. It must be the case with every man who is more entertained with nature than with books. The present dispute has given me a fresh instance of it. Glisson having been quoted, I considered what he had advanced upon this subject, and had the pleasure and mortification to find that he gave † exactly the same account both of transudation and of absorption: so that I can no longer call it, what I really believed it to be, a *new opinion*, but *Glisson's revived and confirmed*; for in him it was *mere opinion*, and accordingly was overlooked or rejected by his successors, as happened to the doctrine of the circulation in the writings of Servetus and Cæsalpinus.

I come next to consider our author's sentiments as to this question: *How far the right belongs to him who first puts a discovery into print?* I know of no act of parliament, indeed, about the matter; but will venture to say, that all men will think alike upon the question. And I cannot drop the subject without smiling at the weakness of quoting precedents for establishing injustice.

I am come to the last article; and every thing is of a piece. “Perpetual disputes with Pott, Haller, Albinus, Monro, or perhaps twenty more, will prove very troublesome, and will at last redound very little to his honour.”

Professor Monro has been pleased to put his son into very good company, but he must give me leave to make some distinction.

Pray how came the names of Haller and Albinus here, two names respected by all the world, and by nobody more than myself? To mortify me still more, he might have added Morgagni and Winslow; and

* See his dedication to the college. “*Meam de motu & usu cordis & circuitu sanguinis sententiam, E. D. D. antea sæpius in prælectionibus meis anatomicis aperui novam, &c.*”

† Anat. Hepat. Lond. 1654. pag. 402. 448. & 456.

and then every body that does not know me, would have supposed that I had treated all these great men in a manner very unbecoming one who professes himself to be their admirer, not their rival. If, in a few instances, I have ventured to dissent from such great authorities, my dissent proceeded from nothing but freedom of enquiry and conviction of mind. Surely then, if Professor Monro alludes to any thing of this kind, he does me an injury. Baron Haller, and Professor Albinus, ever diligent and successful in study, have, in their late publications, anticipated me in some things. I have said so in my public lectures, where it was well known what doctrines I had taught, and how long I had taught them. But I never made a dispute of these things with them, nor they with me: and I never did accuse either of them of treating me unfairly in any sense, nor ever had reason to think that either of them had done so. If then Professor Monro alludes to these anticipations, when he draws and publishes a character with such freedom, he certainly acts unjustly. When I think of this, and of his years, I feel something different from resentment, and would therefore wish to have done with him: but in justice to myself, I must give an account of some parts of my lectures, which misrepresentation might stile disputes, and which I presume were pointed at by Professor Monro senior.

C H A P. VIII.

Of the Discovery of the MEMBRANA PUPILLARIS, and of the Insensibility of Tendons, &c.

Natural curiosity makes students wish to know something of the history of anatomical discoveries and opinions. It is not only entertaining but useful to see by what pursuits and steps an improvement was made: it gives clearer ideas of the subject, makes a stronger impression upon the memory, shews the most probable road to improvement in similar enquiries, and raises emulation. Upon these accounts the historical part is introduced into anatomical lectures: and if a teacher thinks he has made any improvements himself, he will naturally give an account of them for the reasons above-mentioned, and for another reason too, which I need not mention.

In treating historically of the *membrana pupillaris* in my lectures, I have generally

generally given the honour of the discovery to Dr. F-----s S-----s. At the same time I did justice to Wachendorf, Haller, and Albinus, and do sincerely believe that they all made the discovery fairly. I did that justice, indeed, to Albinus before he published any thing upon the subject, and told my audience, that when I paid my respects to him in Holland, he not only shewed me a preparation, but an engraved figure of that vascular membrane, and then told me that he had known it many years *. However, upon inquiry, I understood that Dr. S----- had found it out before any of these gentlemen, and had shewn it to all his acquaintance who had any curiosity for such subjects. 'Tis true, he never put it into print, but it is not the less true that he found it out, and that he has long had that and all parts of the eye finely prepared and preserved, and elegantly expressed in drawings.

I have

* Speaking of this membrane (Academ. Annotat. lib. 3. pag. 82.) he says, "Equidem scio, reperisse me — — in infante illo, in quo primum vasa crystallini implevi." Now, in justice to this great anatomist, I must declare that I believe this, both because he asserts it, and because I know from the circumstances, it was hardly possible he could miss taking notice of it in that child. I have always observed, both in the human body, and in the quadrupede, that there is a great resemblance to one another in the vessels of the *capsula crystallini* and of the *membrana pupillæ*. In an injected *fœtus* I always find both nearly in the same state: if one be filled only with the blood that is driven before the injection, so is the other; if one be filled partly with the injection, and partly with blood, the other is in the same condition; if one by good fortune be finely and minutely filled by injection, the other is so too; if one be hurt by extravasations, the other is commonly in the same state; and when the *fœtus* is so near its full time, that the one cannot be injected, neither can the other. Therefore I say, I know that the *membrana pupillæ* was injected by Albinus in that child; and, if so, I cannot think that it could escape his intelligent and inquisitive eye.

I do not know that any person has taken notice of a circumstance relating to the vessels of the *membrana pupillæ* and of the crystalline *capsula*, which I have observed and can demonstrate by injections, both in the human *fœtus*, and in that of the quadrupede; and as I am upon the subject, I will give the observation here in a few words.

The artery of the crystalline *capsula* does not terminate at the great circle of that humour. Its small branches pass that circle, and run a very little way on the anterior surface of the crystalline humour before the points of the ciliary processes; then they leave the humour, and run forwards, supported on a very delicate membrane, to lose themselves in the *membrana pupillæ*.

The artery, therefore, that passes through the body of the vitreous humour, goes first to the crystalline *capsula* and then to the *membrana pupillæ*.

The *membrana pupillæ* receives two different sets of arteries, one larger from the *iris*, and the other much smaller, but very numerous, from the crystalline *capsula*.

When the *membrana pupillæ* exists, there is a fine vascular membrane all around, which passes in the posterior aqueous chamber, from near the edge of the crystalline humour to the edge of the *pupilla*.

I have chosen this instance to shew what must naturally be passing at anatomical lectures, where there is a regard for truth: and it is the more to my purpose, as it is *well known here* that I could have no motive but the love of truth for introducing this part of anatomical history in my lectures. And was any person to represent such an anecdote as *a dispute with Albinus or Haller that would redound little to my honour*, it would only prove his ignorance of the subject, or malevolence towards me.

But if it be right to do justice in this way to another person, surely it cannot be wrong to do the same sort of justice to one's self. Anatomists of the fairest character in every age have done so; and among living authors, Albinus and Haller are respectable authorities, and a sanction to the practice.

In my lectures I commonly took notice that Professor Albinus had anticipated me in some things, and particularly with regard to the second set of teeth. It was a fact as well known here, and as capable of being proved as that I had read lectures ever since the year 1746; and therefore it can never *redound to my dishonour*, nor could I on this account be represented, but by malevolence or ignorance, as having any dispute with Professor Albinus, who possibly might, and I suppose, really did know these things long before me.

Ever since I read lectures, I have been of opinion that the *periosteum*, *dura mater*, tendons, and ligaments, were altogether insensible, or at least were endowed with a very small degree of sensibility; and have always taught that doctrine. I considered this question as may be seen in the MS of my lectures, when I treated of the *periosteum* and of the tendons; and particularly when of the ligaments. In the first two years, viz. 1746 and 1747, I proposed the opinion with reserve, well knowing the opposition that prejudice raises against new doctrines; but ever since the year 1748, I have spoken of this opinion with more firmness. The account which I used to give was a little different at different times, as it was for the most part given from memory, but was always to the following purpose.

Periosteum. "Authors generally suppose the *periosteum* to be extremely sensible, and therefore plentifully supplied with nerves*. But I believe no anatomist can confirm this doctrine by dissections of the nerves; and some cases in surgery, which I have attended to, seem
" to

* Among these authors I might have mentioned Professor Monro senior; and indeed, I believe, sometimes I did mention him.

“ to disprove the supposition. I have often seen the *periosteum* cut and
 “ scraped, both on the scull and in the limbs, without the patient’s
 “ seeming to be at all in pain. The argument drawn from the tendons
 “ diffusing themselves into the *periosteum*, and furnishing it with nerves
 “ for sensation, will have no weight, if it be found as difficult to trace
 “ nerves into the tendons as into the *periosteum*, and if the tendons them-
 “ selves should be found to be insensible. The argument taken from the
 “ excruciating pain in some nodes is inconclusive, as we frequently see
 “ large ones that are perfectly indolent. And the supposition that the
 “ sensibility of the *periosteum* was necessary, considering it as a guard to
 “ the insensible bone which it incloses, cannot have any weight when
 “ we reflect that both are inclosed by the skin, one of the most sen-
 “ sible parts of the whole body. Whoever has felt a smart pinch
 “ of the skin, will readily allow that there is no occasion for any thing
 “ besides the skin to keep us from wantonly bruising or breaking our
 “ own bones. I imagine that all the white, glistening, inelastick sub-
 “ stances, viz. *periosteum*, tendons, ligaments, and *dura mater*, are of
 “ the same nature; and therefore I shall more fully consider this pro-
 “ perty of sensibility when treating of *ligaments*.

Tendons. “ They have been supposed to be extremely sensible. On
 “ the contrary, I believe that in a healthy and natural state they have
 “ little or no feeling. But we shall take this matter into consideration
 “ when we come to *ligaments*.

Ligaments. “ These too have been thought to be abundantly supplied
 “ with nerves, and to be sensible in living bodies: yet, from my own
 “ observation and reflection, I cannot help being of opinion that they
 “ have hardly any sensibility at all. Ligaments and tendons are made
 “ of the same sort of glistening, inelastick fibres; and differ only in
 “ this respect, that ligaments bind bones to bones, whereas tendons
 “ bind muscles to bones; so that tendons are in effect ligaments. Thus
 “ for example, in the knee it is the same sort of substance that binds
 “ the *musculi extensores* to the *patella*, and the *patella* to the *tibia*: so
 “ much the same, that many parcels of fibres are common to both,
 “ being continued over the surface of the *patella*, from the one, to
 “ make a part of the other: yet it is called tendon above the *patella*,
 “ and ligament below it; and between these, where the fibres cover
 “ the surface of the bone, it is called *periosteum*. Tendons, ligaments,
 “ *aponeuroses*, *periosteum*, and *dura mater*, are so evidently of the same
 “ nature, that we must suppose them endued with nearly the same share

“ of feeling*. In wounds of the hands and feet I have often seen
 “ tendons partly divided or cut quite through, where the patient com-
 “ plained of no remarkable pain, either at the time of receiving the
 “ injury, or afterwards in the course of dressing and wiping the fore.
 “ I have known the *tendo achillis* torn quite asunder without pain. I
 “ was consulted by an Italian gentleman, who had a gradual and par-
 “ tial rupture of the *tendo achillis*. It happened to him at several
 “ different times, and first of all when he was walking in the mall.
 “ He was always sensible of something giving way at the time, but
 “ never had any pain or any other sensation, except a kind of weakness
 “ and awkwardness in the motion of the part. The most unexception-
 “ able proof that I can give of this doctrine, is the case of Mr. Ser-
 “ jeant Ranby. He was very near losing his life, as most of you may
 “ know, from puncturing his finger with a pair of scissors†. When
 “ the part was laid open, the tendons of the *flexores* were exposed, and
 “ appeared to be perfectly sound. He cut them both through with his
 “ own hand at one stroke of a pair of scissors; and when I asked him
 “ about his feeling he assured me, that except the jarring of the instru-
 “ ment, which hurt him a little on the lips of the wound, he had no
 “ more pain or feeling of any kind, than if he had cut a cord or any thing
 “ that was not a part of his own body. He thought my opinion well
 “ grounded, and said his own case had given him the strongest convic-
 “ tion that tendons were insensible, and that it was a circumstance which
 “ he had particularly attended to. Now if we suppose a tendon pos-
 “ sessed of any degree of feeling, we can hardly conceive any thing
 “ more likely to give pain, than the bruising pinch of a pair of scissors,
 “ in cutting so thick and so hard a substance.

“ That the *dura mater* has little or no feeling I am convinced, from
 “ having seen it, in two different patients, laid open by a crucial incision
 “ in

* When I was a student in St. George's hospital, in the year 1741 and 1742, I had the
 pleasure of attending Dr. Nicholls's courses of anatomy. All his pupils must remember his
 “ division of ligaments into *inelastic*, *inelastic-degenerated*, and *elastic*. The *inelastic*, when stretched,
 “ breaks before it be sensibly lengthened; is of a white colour, and has a peculiar glisten-
 “ like pearl or polished silver. It loses that colour by boiling, and then becomes elastic;
 “ but grows shorter in the same proportion. Of this substance are tendons, *aponeuroses*,
 “ the *periosteum*, *dura mater*, and many of the ligaments of the bones.” These ideas I first
 received from that ingenious and elegant anatomist, and my own dissections and observation
 confirmed his doctrine. It was therefore very natural for me to conclude that all these
 parts in the body were *insensible*, as soon as I had observed that any one of them was so.

† This was in the year 1748.

“ in the operation of the trepan. Though both of them complained
“ very much when scalped, neither of them shewed any symptoms of
“ being disturbed when the *dura mater* was cut.

“ Whenever there has been extraordinary pain at the time of the
“ operation from bleeding in the arm, I take it for granted that the
“ lancet has touched a nerve, and not the tendon, as is commonly sup-
“ posed : and we all know that the alarming symptoms which sometimes
“ happen after bleeding, are oftener owing to some circumstances of the
“ patient's constitution, than to any unfortunate or unskilful manner of
“ executing the operation.

“ If we allow that tendons, ligaments, &c. have any, the smallest,
“ degree of sensibility, we can easily imagine from analogy that in-
“ flammation, and other circumstances of a disease, may raise that sen-
“ sibility to a very high degree. A certain degree of cold, we know,
“ exasperates pain ; and heat mitigates it in all parts of the body.
“ There may be many other things less known that have the same or
“ much greater influence on pain, and that may act in particular parts
“ of the body only, or be pretty equally diffused over the whole.

“ Without having taken the pains of actually tracing the history of
“ the opinion, I think I can guess how it came about that tendons, &c.
“ were thought to be very sensible. In the first place, it must have
“ been observed in surgery, that all such parts are apt to suppurate in
“ an unkindly manner ; to produce sloughs generally before they gra-
“ nulate when exposed to the air ; and that frequently dreadful in-
“ flammations and fevers come on from apparently slight injuries in
“ such parts. Thence it would seem natural to conclude, that those
“ parts are very irritable, sensible, and full of nerves, the organs of
“ sensation. Another thing that would naturally mislead people into
“ the opinion was this : all these parts were constantly by anatomists
“ called *nervous* parts. The Greeks gave us our anatomical language,
“ and used the word *νευρον*, not only to signify what we now call a *nerve*,
“ but a *tendon* likewise and a *ligament*. It was the name given to the
“ *genus* of which there were three *species* ; and, for this reason, the term,
“ *nervous parts*, in Greek writers, signifies equally *nervous*, or *tendinous*,
“ or *ligamentous* parts of the body. Thus tendinous expansions were
“ called *aponeuroses* by the Greek writers, and by the moderns too.
“ It is from the same source that we have taken our expression *a ner-
“ vous arm*, &c. signifying sinewy, or strong, and *nervous stile*, expressing
“ force and energy.”

The first time I ever knew of Baron Haller's having any particular opinion upon the subject, was on paying a visit to Dr. Peter Shaw, who gave me Castell's *thesis*, printed in 1753; and some time after, I had the pleasure of reading the Memoire in the second volume of the *Acta Gotting.* published in the same year*. From that time, at my lectures, I treated the insensibility of tendons, &c. as my own observation, expressed my satisfaction at its being confirmed, and extended to other parts by Baron Haller; and after giving a general account of his observations with commendation and respect, I commonly made remarks to the following purpose.

REMARK 1. "After all I suspect that Baron Haller has perhaps gone too far in concluding that these parts have absolutely *no sense* of feeling. Experiments made on brutes cannot ascertain the fact. It must be done by observations made on living human bodies, where we can be informed of the more obtuse and gentle, as well as of the more painful and exquisite sensations. The case of Mr. Ranby seems to me more conclusive than any hitherto related of the human body; and

* As I really have not, nor ever had, a dispute with Baron Haller, and as I believe he would upon every occasion be ready to do me justice, I would avoid every thing that could be construed disputing with him. Yet the reader would no doubt wish to see here in a few words how this opinion of insensibility stands as to time betwixt Baron Haller and myself.

I conceived the opinion in 1741 or 1742, when I attended St. George's hospital; taught it in my lectures in 1746; confirmed it by serjeant Ranby's case in 1748; and first learned that Baron Haller was nearly of the same opinion in 1753.

Baron Haller conceived the first opinion about the sensibility of tendons in May, 1748 (a); and the first experiment which he made on purpose to determine the point was in November, 1750 (b).

The first experiment which he mentions on the *periosteum* was made in November, 1750 (c).

His first upon ligaments was in December, 1750 (d).

The first observations which he made upon the insensibility of the *dura mater* were with Zinn (e), when they were making experiments upon the *corpus callosum*. Baron Haller says this was in January, 1748. Perhaps there may be some mistake or typographical error in this date; for from the following *exordium* of Zinn's *thesis*, we should naturally believe that the experiments had been made late in the spring, or in the summer, 1759. *Cum mihi scribendum esset tempore a dissectionibus anatomicis libero — — decrevi experimenta quædam capere circa corpus callosum, cerebellum & duram matrem.*

The first experiments that were made expressly in order to try the sensibility of the *dura mater* were in November, 1750 (f).

(a) Mem. sur la Nat. sensible, &c. Vol. I. p. 130. (b) Ibid. p. 116. (c) Ibid. p. 139. (d) Ibid. p. 140. (e) Ibid. p. 151, 152. (f) Ibid. p. 152.

“ and yet, when all is laid together, I would not venture to pronounce
 “ such parts to be absolutely void of sense in a sound state, or incapable
 “ of more acute feeling in particular diseases.

REMARK 2. “ Baron Haller appears to be led into an error in surgery.
 “ From his experiments and observations he seems to think that wounds
 “ and punctures of tendons and ligaments, and penetrating wounds in
 “ the joints, will be found to be attended with as little danger, and to
 “ heal as kindly as similar wounds in fleshy parts. The danger of a
 “ wound will not be found to be proportioned to the pain which the
 “ patient feels when the wound is inflicted; that pain being momentary
 “ for the most part, or transient, and of little consequence. And cures
 “ that happen in the body of the quadruped must be applied with great
 “ caution to the human body. From many cases that have come under
 “ my own observation, as well as those which I have learned from
 “ others, I am so well convinced of the great danger of punctures in
 “ tendinous and ligamentous parts, more especially about joints, that I
 “ think it my duty to put you upon your guard: and particularly
 “ to caution you against cutting into the cavity of a joint, unless
 “ there be very urgent reason. Sometimes, indeed, these wounds will
 “ heal up very kindly; and such favourable cases have made some sur-
 “ geons look upon them as not particularly dangerous; but more ge-
 “ nerally, even when the habit appears to be healthy, they are followed
 “ with violent pain and inflammation, and with a very bad species of
 “ fever, and with large suppurations. At this time patients are often
 “ delirious; often convulsed. Sometimes they are soon carried off by
 “ the violence of the fever: sometimes death is more slow, as if it were
 “ partly brought on by the fever, and partly by the immense discharge.
 “ And when they do recover, it is almost always with the loss either
 “ of the limb, or at least of the motion of the joint: and generally
 “ the whole habit is so much vitiated, that it is a long time before
 “ they recover perfect health*.

“ Perhaps from the obvious structure of a joint one reason may be
 “ assigned for an inflammation being more painful and more mischievous
 “ in its consequences here, than in many other parts; there is a want of
 “ room for the parts to swell, and yet to be tolerably at their ease. The
 “ cavity of a joint is so fully possessed in the natural state, and the li-
 “ gaments so tight and so strong, that when the deep-seated parts inflame
 “ and

* Here cases were mentioned.

“ and swell, they compress one another in the same proportion : and that
 “ the compression of an inflamed part will aggravate pain, and give fury to
 “ the disease, needs not be explained in a country where tight bandages
 “ have been so happily exploded.”

If all this should be reckoned *a dispute with Haller*, if it should prove very troublesome, and at last redound very little to my honour, as Professor Monro gives out, anatomists must be very cautious in their improvements and communications.

But I have the pleasure to find that Baron Haller has not taken his opinion of me from the two Professors ; for in the very last piece that I have seen of his writing, where he is treating of the dispute about the sensible and insensible parts, and where he is complaining with great freedom of some other men, he mentions my name, and my opinion, in a very different manner *. And I am the more proud of the good opinion which he is pleased to entertain of me, as I have never taken any pains to clear myself of the aspersions. On the contrary, I rather avoided a fair opportunity that fell in my way ; and I did so, because I felt myself mortified at the thought of being obliged to justify myself against an accusation so ill grounded, and so strongly marked with partiality.

C H A P. IX.

Of the Rupture, in which the *Testis* is in contact with the Intestine.

THE only dispute, which Professor Monro sen. can possibly accuse me of having with Mr. Pott, relates to that particular species of rupture, in which the intestine is found in contact with the *testis* ; and, I presume, is what the Professor is afraid may redound to my dishonour. I shall endeavour fairly to represent the case ; and shall then most readily submit to the judgment of the public.

Some time about the year 1748, Mr. Sharp asked me in conversation, if, in dissecting ruptures, I had ever found the intestine in the same bag, and in contact with the *testis*. I told him, I never had found it so, and did not think it possible. He said he had met with it three times, if
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* Mem. sur les Part. sensib. & insensib. Tom. IV. p. 37.

he was not very much deceived; but, that, in two of those instances, he could not be so positive about the fact, because the observation was made in performing the operation for the bubonocoele on a living body. He did me the honour to desire that we might examine together the first ruptured subject that either of us should meet with. A few weeks after this a subject was brought to me, which was ruptured on both sides. The size and shape of the tumour were almost exactly the same in both, and the protruded bowels were fallen down as far as the lowest part of the *scrotum*. I examined them in Mr. Sharp's presence and under his direction; and as soon as the hernial sac of the right side was laid open, we saw the *testis* lying bare, in the bottom of its cavity. The *tunica albuginea* and the naked *epididymis* were seen so distinctly, that there was no room for a moment's doubt. Then we dissected the rupture of the left side; and there it was as indisputable that the bottom of the hernial sac was situated upon the outside of the *tunica vaginalis propria*, or, in other words, that these two bags were distinct, and without any communication; and that the intestine in such a rupture could not have come into contact with the *testis*, unless a laceration had been produced both in the hernial sac, and in the *tunica vaginalis propria*. We therefore concluded that such a laceration of those bags had actually happened to the rupture on the right side, and that it must happen in all ruptures where the *testis* is found in contact with the intestine.

At this time both Mr. Sharp and myself were considering the parts concerned, only as they really are in sound adult bodies: and whenever a rupture is produced under such a state of those parts, it cannot be otherwise; that is, either the hernial sac and *tunica vaginalis propria* must be distinct cavities, or the bowels must have forced their way through both bags, and thus have come into contact with the *testis*. Mr. Sharp taught this doctrine afterwards in his writings*, as I did in my lectures. I preserved the two ruptures, and occasionally shewed them to illustrate the doctrine in my lectures, and always told my audience what I have been now relating, as a piece of justice to Mr. Sharp.

* Critical Enquiry, Lond. 1750. pag. 3. "It is evident to me, that notwithstanding
" the *peritonæum* may at first fall down with the *viscera*, yet in length of time it may also
" be ruptured, because I have found the *intestine* and *omentum* within the *tunica vaginalis* of
" the testicle, and in contact with the testicle itself, which they could not possibly have
" been, if they were enveloped in a portion of the *peritonæum*: however, this circumstance
" occurs but rarely, for we usually find, &c."

Soon after this, Mr. Cheselden, who was then composing his remarks on Le Dran's Surgery, saw these two ruptures, and desired to have a drawing, or rather indeed a sketch of them, which he engraved and published. In the explanation of the figures, he did me the honour to mention me, and declared himself to be of the same opinion, as to the manner in which the intestine had got into contiguity with the *testis**.

In the latter end of the year 1755, when I first had the pleasure of reading Baron Haller's observations on the *hernia congenita*†, it struck my imagination that the state of the *testis* in the *fœtus* and its descent from the *abdomen* into the *scrotum* would explain several things concerning ruptures and the *hydrocele*, and particularly that observation which Mr. Sharp had communicated to me, viz. that in ruptures the intestine is sometimes in contact with the *testis*. I communicated my ideas upon this subject to my brother, and desired that he would take every opportunity of learning exactly the state of the *testis* before and after birth, and the state of ruptures in children. We were both convinced that the examination of those facts would answer our expectation, and both recollected having seen appearances in children, that agreed with our supposition, but saw now that we had neglected making the proper use of them.

In the course of the winter, my brother had several opportunities of dissecting fœtuses of different ages, and of making some drawings of the parts; and all his observations agreed with the ideas I had formed of the nature of ruptures, and of the origin of the *tunica vaginalis propria* in the *fœtus*. But till those observations were repeated to his satisfaction, and were sufficiently ascertained, he desired me not to mention the opinion in my lecture; and therefore, when treating of the coats of the *testis*, and of the situation of the hernial sac, &c. I only put in this temporary caution, that I was then speaking of those things as they are commonly in adult bodies, and not as they are in the *fœtus*: and at last, when I was concluding my lectures for that season in the end of April, 1756, with a course of the surgical operations, I gave a very general account of my brother's observations, and shewed both the drawing of Fig. II. which was then finished, and the subject from which it was made.

Some

* Le Dran's Operations, translated by Mr. Gataker, with Mr. Cheselden's remarks, Lond. 1749, page 463. "E, the sac of the *hernia intestinalis* which had communicated itself to the testicle——The present cases I had from Mr. Hunter, &c."

† Alberti Halleri Opuscul. Patholog. Laufan. 1755, 8vo. pag. 53, &c.

Some time in May or June following, Mr. Pott presented me with his Treatise of Ruptures. In the preface I found that he had done me the honour of adding my name to a very respectable list; and I imagined that this compliment was meant as a very kind return for the respect which I had wished to shew him upon every occasion, and particularly for what had passed between us some time before, at a meeting for examining the nature of ruptures, and the state of the parts concerned: for, when he began to compose his treatise (as I presume, because, though he said nothing to me of such an intention, I soon after heard of it among his friends) he desired that we might examine those things in the first proper subject which I could apply to that purpose. My brother dissected the parts on both sides of a body, in the same manner as they were commonly prepared for my lecture, and so as to demonstrate my ideas as clearly as possible. We examined the parts with attention, both in the fresh subject, and in some preparations of *herniæ*; and those who have attended my lectures may imagine what I demonstrated, and what I said. So far as I could judge, I had the satisfaction of finding that we thought alike upon most points.

In perusing the book itself, I was sorry to meet with the following passage, (page 13) “ If the testicles of a *fætus* were down in the *scrotum*, dependent from the spermatic chord, as they are in an adult, they would in some postures and dispositions of the child at the time of parturition be very liable to be hurt; to prevent which, and possibly for other reasons also, the testicles of a *fætus*, during its residence in the *uterus*, lie within the *abdomen*, behind the *peritonæum*, defended by the bone.

“ Soon after birth, when the lungs come to be distended with air, and press on the diaphragm, when the muscles of respiration act, and those of the *abdomen* begin to squeeze the contents of the belly, the testicles are pushed out through the apertures in the abdominal muscle (called the rings) into the upper part of the *scrotum*: this passage of the testicle from the belly into the *scrotum*, I take to be the principal cause of the ruptures of infants; for the ring or aperture being by this means dilated, a portion of caul or gut has an opportunity of slipping through, before the aperture has had time to contract itself again, and which protrusion will be forwarded by the continual efforts of the child in crying.

“ This has always appeared to me to be the case-----”

I say, I was sorry to see this passage in my friend's performance, but

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took no notice of it to the author, when I thanked him for his present, or to any other person. The subject appeared to me to be too delicate for conversation; and the reader will no doubt think so, when I tell him in what light it appeared to me. The first part is certainly erroneous with regard to the fact which is advanced; and the reasoning is not so solid as might have been expected. *The testes* of a *fœtus* do not lie within the *abdomen* till after its birth; they fall down before that period: and surely we must allow that the *testes* are more in danger from accidents after, than in the time of birth; and that, if their situation was to be determined by *protection* and *danger*, they ought to be going up, just at the time when they are coming down.

The second part of the passage seems to be taken from Haller; at least it happens to be precisely what that author had published in the preceding year. His words are “*Neutram causam (sc. herniarum) exclusam velim, profundius tamen sæpe latere radices, ex his observationibus adparebit, ex quibus constat in ipso fœtu non valde raro jam natam herniam reperiri, vacuum equidem, sed quam ex minimis causis intestino repleti oporteat.-----In fœtu enim---testes in cellulosa tela lumborum sedent, proxime renum---Descendunt inde---sensim &---in scrotum adveniunt, semper retro peritonæum, &c. Causa hujus progressus videtur in respirationis vi, & in musculorum abdominis potestate poni.---Herniarum, ni fallor, congenitarum, modus hinc elucescit, quo generantur. Patulus est processus peritonæi, &c. Cum autem his in corporibus testes eodem cum intestinis sacco omnino contineantur, nihil est singularis sive inexpectati, si ea in apertum saccum, a levi vi depressa fuerint---Annon id suadet notissima observatio, longe plerisque herniosos in ætate infantili id vitium contraxisse---Nullum adeo fere mihi dubium superest, quin prima in origine testis in abdomine sedeat, deinde vi respirationis, clamoris & nixuum paulatim, &c.*” Now if the reader will consider that Baron Haller’s *Opuscula Pathologica*, which contained this curious observation, were published in the year preceding the publication of Mr. Pott’s treatise, and were in every body’s hands here*, he will not be surprised that I felt some uneasiness for my friend, at reading the last part of the passage quoted, viz. “This has always appeared to me to be the case.”

Upon

* This work of Haller had not only been generally read in the original, and approved of here, but even its English translation was advertised on the 20th of February, 1756, that is, some months before Mr. Pott’s publication.

Upon reading a few pages more, I found that whether he had always been of that opinion, or had forgot that it was likewise Haller's, he could not then account for the contiguity of the *testis* and intestine in some ruptures. "The case quoted by Mr. Sharp," says he, page 21, "of the intestine being found in contact with the testicle being an accidental thing, and to be ranked as such, or as one of the *lusus naturæ*." Therefore I concluded that what I had taught at my lecture in the latter end of April, relating to the *hernia congenita*, could not have come to his knowledge before the first part of his book was printed.

My brother continued his inquiry, and by the autumn had ascertained what I shall next present to the public in his name. Afterwards I shall resume my narrative.

"Observations on the State of the *Testis* in the *Fœtus*, and on the *Hernia Cogenita*, by Mr. JOHN HUNTER.

"Until the approach of birth, the *testes* of the *fœtus* are lodged within the cavity of the *abdomen*, and may therefore be reckoned among the abdominal *viscera*.

They are situated immediately below the kidneys, on the forepart of the *psoæ* muscles, and by the side of the *rectum*, where this intestine is passing down into the cavity of the *pelvis*: for in the *fœtus* the *rectum*, which is much larger in proportion to the capacity of the *pelvis*, than in the full grown subject, lies before the *vertebræ lumborum* as well as before the *os sacrum*. Indeed the case is pretty much the same with regard to all the contents of the *pelvis*: that is, their situation is much higher in the *fœtus* than in the adult; the sigmoid flexure of the *colon*, part of the *rectum*, the greatest part of the bladder, the *fundus uteri*, the fallopian tubes, &c. being placed in the *fœtus* above the hollow of the *pelvis*, in the common or great abdominal cavity.

At this time the shape or figure of the *testis* is much the same as in the adult, and its position or attitude is the same as when it is in the *scrotum*: that is, one end is placed upwards, the other downwards; one flat side is to the right, the other to the left; and one edge is turned backwards, the other forwards. But as the *testis* is less connected with the surrounding parts while it is in the loins, its position may be a little variable. The most natural seems to be when the anterior edge is turned directly forwards; but the least touch of any thing will throw that edge

either to the right side, or to the left, and then the flat side of the *testis* is turned forwards.

It is attached to the *psoas* muscle all along its posterior edge, except just at its upper extremity. This attachment is formed by the *peritonæum*, which covers the *testis* and gives it a smooth surface, in the same manner as it envelops the other loose abdominal *viscera*.

The *epididymis* lies along the outside of the posterior edge of the *testis*, as in older bodies, but is larger in proportion, and adheres backwards to the *psoas*. When the *fœtus* is very young, the adhesion of the *testis* and *epididymis* to the *psoas* is very narrow; and then the *testis* is more loose, and more projecting: but as the *fœtus* advances in months, the adhesion of the *testis* to the *psoas* becomes broader and tighter.

The vessels of the *testis*, like those of most parts of the body, commonly rise from the nearest larger trunks, viz. from the *aorta* and *cava*, or from the emulgent.

The artery rises generally from the forepart of the *aorta*, a little below the emulgent artery; and often from the emulgent itself, especially in the right side of the body; which may happen the rather, because the trunk of the *aorta* is more distant from the right *testis* than from the left. Sometimes, but much more rarely, the spermatic artery springs from the phrenic, or from that of the *capsula renalis*. Besides the artery which rises from the *aorta*, or emulgent, &c. the *testis* receives one from the hypogastric artery, which is sometimes as large as the other. It runs upwards from its origin, passing close to the *vas deferens*, in its way to the *testis*. The superior spermatic artery sometimes passes before the lower end of the kidney. Both these arteries run in a serpentine direction, making pretty large but gentle turnings; both are situated behind the *peritonæum*, and both run into the posterior edge of the *testis*, between the two reflected *laminae* of that membrane, much in the same manner as the vessels pass to the intestines between the two reflected *laminae* of the *mesocolon* or mesentery.

The veins of the *testis* are analogous to its arteries. The superior spermatic vein (to begin with its trunk) rises commonly in the following manner; on the right side from the trunk of the *cava* a little below the emulgent, and on the left side from the left emulgent vein. The reason of this difference between the right and left spermatic vein, no doubt, is because the *cava* is not placed in the middle of the body: so that by the rule of ramification, which is observed in most parts of the body, the *cava* is the nearest large vein of the right side, and the emulgent is the
nearest

nearest large vein of the left side. But the difference is inconsiderable; and accordingly we sometimes find the right spermatic vein coming from the right emulgent vein, and several other varieties, which, so far as I can observe, follow no precise rule. There is likewise a spermatic vein, which rises from the internal iliac, and runs up to the *testis* with the inferior spermatic artery. Both the spermatic veins run behind the *peritonæum* with their corresponding arteries, and go into the posterior edge of the *testis*, where they are lost in small branches.

The nerves of the *testis*, like its blood vessels, come from the nearest source; that is, from the abdominal plexuses of the intercostal; especially the inferior mesenteric *plexus*. They run to the *testis*, attending upon its blood-vessels, and are dispersed with them through its substance. The *testis* therefore, with respect to its nerves, may be reckoned an abdominal *viscus*; and this observation will hold good, when applied to the full-grown subject, as well as to the *fœtus*: for those branches of the lumbar nerves, which are commonly said to be sent to the *testis*, passing through the tendon of the external oblique muscle, in reality go not to the *testis* itself, but to its exterior coverings, and to the *scrotum*.

The *epididymis* begins at the outer and posterior part of the upper end of the *testis*, immediately above the entrance of the blood-vessels. There it is thick, round, and united to the *testis*; as it passes down, it becomes a little smaller and more flat, and is only attached backwards to the *testis*, or rather indeed to its vessels, for it lies loose against the side of the *testis* forwards: and at its lower end it is again more firmly attached to the body of the *testis*; so that in the *fœtus* there is a cavity or pouch formed between the middle part of the *testis* and the middle part of the *epididymis*, which is more considerable than what is commonly observed in full-grown subjects. As the body grows, the *epididymis* adheres more closely to the side of the *testis*. The greatest part of the *epididymis* is made up of one convoluted canal, which becomes larger in size and less convoluted towards the lower end of the *epididymis*, and at last is manifestly a single tube running a little serpentine. That change happens at the lower end of the *testis*, and there the canal takes the name of *vas deferens*.

This duct is a little convoluted or serpentine in its whole course, but is less so as it comes nearer to the bladder; instead of running upwards from the lower end of the *testis*, as it does at a more advanced period of life, in the *fœtus* at this age it runs downwards and inwards in its whole course; so that it goes on almost in the direction of the *epididymis*, of which

which it is a continuation. It turns inwards from the lower end of the *epididymis*, under the lower end of the *testis*, and behind the upper end of a ligament or *gubernaculum testis*, which I shall presently describe; then it passes over the iliac vessels and over the inside of the *psoas* muscle, somewhat higher than in adult bodies; and at last goes between the *ureter* and bladder towards the *basis* of the prostate gland.

At this time of life the *testis* is connected in a very particular manner with the *parietes* of the *abdomen*, at that place where in adult bodies the spermatic vessels pass out, and likewise to the *scrotum*. This connection is by means of a substance which runs down from the lower end of the *testis* to the *scrotum*, and which at present I shall call the ligament, or *gubernaculum testis*, because it connects the *testis* with the *scrotum*, and directs its course in its descent. It is of a pyramidal form; its large bulbous head is upwards and fixed to the lower end of the *testis* and *epididymis*, and its lower and slender extremity is lost in the cellular membrane of the *scrotum*. The upper part of this ligament is within the *abdomen*, before the *psoas*, reaching from the *testis* to the groin, or to where the spermatic vessels begin to pass through the muscles. Here the ligament runs down into the *scrotum* precisely in the same manner as the spermatic vessels pass down in adult bodies, and is there lost. The lower part of the round ligament of the *uterus* in a *fœtus* very much resembles this ligament of the *testis*; and may be plainly traced down into the *labium*, where it is imperceptibly lost. That part of the *ligamentum testis*, which is within the *abdomen*, is covered by the *peritonæum* all around, except at its posterior part, which is contiguous to the *psoas*, and connected with it by the reflected *peritonæum*, and by the cellular membrane. It is hard to say what the structure or composition of this ligament may be. It is certainly vascular and fibrous, and the fibres run in the direction of the ligament itself. It may be muscular; and I am inclined to believe that it is in part composed of the *cremaster* muscle turned inwards, and running upwards to join the lower end of the *testis*. The following observations seem to render this hypothesis probable.

In the hedge-hog the *testes* continue through life to be lodged within the *abdomen*, in the same situation as in the human *fœtus*; and they are fastened by the same kind of ligament to the inside of the *parietes* of the *abdomen* at the groin. Now, in that animal, I find that the lowermost fibres of the internal oblique muscle, which constitute the *cremaster*, are turned inwards at the place where the spermatic vessels come out in other animals, making a smooth edge or lip by their inversion; and that then they

they mount up in the ligament to the lower end of the *testis*. Sometimes in the human body, and in many other animals, and very often in sheep, the *testes* do not descend from the cavity of the *abdomen* till late in life, or never at all. In the ram, where the *testis* is come down into the *scrotum*, the *cremaster* is a very strong muscle; and, though it be placed more inwards at its beginning, it passes down pretty much as it does in the human body, and is lost on the outside of the *tunica vaginalis*: but in the ram, whose *testis* remains suspended in the abdominal cavity, I find that the same *cremaster* exists, though it is a weaker muscle; and instead of passing downwards, as in the former case, it turns inwards and upwards, and is lost in the ligament which attaches the *testis* to the *parietes* of the *abdomen*, and which in this state of that animal is about an inch and a half in length. In the human *fœtus*, while the *testis* is suspended in the cavity of the *abdomen*, the *cremaster* is so slender that I cannot trace it to my own satisfaction, either turning up towards the *testis*, or turning down towards the *scrotum*.

The *peritonæum*, which covers the *testis* and its ligament or *gubernaculum*, is firmly united to the surfaces of those two bodies; but all around, to wit, on the kidney, the *psoas*, the *iliacus internus*, and the lower part of the abdominal muscles, that membrane adheres very loosely to all the surfaces which it covers. Where the *peritonæum* is continued or reflected from the abdominal muscles to the ligament of the *testis*, it passes first downwards a little way and then upwards, so as to cover more of the ligament than what is within the cavity of the *abdomen*. At this place the *peritonæum* is very loose, thin in its substance, and of a tender or gelatinous texture; but all around the passage of that ligament the *peritonæum* is considerably tighter, thicker, and of a more firm texture. When the abdominal muscles are pulled up so as to tighten and stretch the *peritonæum*, this membrane remains loose at the passage of the ligament, while it is braced or tight all round; and in that case the tight part forms a kind of border or edge around the loose doubled part of the *peritonæum*, where the *testis* is afterwards to pass. This loose part of the *peritonæum*, like the *intro-suscepted* gut, may, by drawing the *testis* upwards, be pulled up into the *abdomen*, and made tight; and then there is no appearance of an aperture or passage down towards the *scrotum*: but when the *scrotum* and ligament are drawn downwards, the loose doubled part of the *peritonæum* descends with the ligament, and then there is an aperture from the cavity of the *abdomen* all around the forepart of the ligament, which seems ready to

to receive the *testis*. This aperture becomes larger when the *testis* descends lower, as if the pyramidal or wedge-like ligament was first drawn down, in order not only to direct but to make room for the *testis* which must follow it. In some fœtuses I find the aperture so large, that I can push the *testis* into it, as far as the tendon of the external oblique muscle.

From this original situation within the *abdomen* the *testis* is afterwards moved to its destined station in the *scrotum*. It is the more difficult to ascertain the exact time of this motion, as we hardly ever know the exact age of our subject. According to the observations which I have made it seems to happen sooner in some instances than in others, but generally about the eighth month. In the seventh month I have commonly found the *testis* in the *abdomen*, and in the ninth I have as commonly found it in the upper part of the *scrotum*.

At the abovementioned period, the *testis* moves downwards till its lower extremity comes into contact with the lower part of the abdominal *parietes*. By this time the upper part of the ligament, which hitherto was within the *abdomen*, has sunk downwards, lies in the passage from the *abdomen* to the *scrotum*, and thus dilates that passage for the reception of the *testis*. The place where the ligament is most confined, and where the *testis* meets with most obstruction in its descent, is the ring in the tendon of the external oblique muscle: and accordingly I think we see more men who have one *testis*, or both, lodged immediately within the tendon of that muscle, than who have one, or both, still included in the cavity of the *abdomen*.

After the *testis* has got quite through the tendon of the external oblique muscle, it may be considered as possessing its determined station; though it commonly remains for some time by the side of the *penis*, and by degrees only descends to the bottom of the *scrotum*. And when the *testis* has descended intirely into the *scrotum*, its ligament is still connected with it, and lies immediately under it, but is shortened and compressed.

Having now given an account of the original situation of the *testes*, of the time of their descent from the *abdomen*, and of the route which they take in their removal to the *scrotum*; I shall in the next place describe the manner in which they carry down the *peritonæum* with them, and then explain how that membrane forms the sac of the *hernia congenita* in some bodies, and the *tunica vaginalis propria* in others.

When

When the *testis* is descending, and when it has even passed into the *scrotum*, it is still covered by the *peritonæum*, exactly in the same manner as when it was within the *abdomen*; and the spermatic vessels run down behind the *peritonæum* there, as they did when the *testis* lay before the *psoas* muscle; and that *lamella* of the *peritonæum* is united behind with the *testis*, the *epididymis*, and the spermatic vessels (besides the *vas deferens*) as it was in the loins; and the *testis* is fixed backwards to the parts against which it rests, and is unconnected and loose forwards, as it was when in the *abdomen*. In coming down, the *testis* brings the *peritonæum* with it; and the elongation of that membrane, though in some circumstances it be like a common hernial sac, yet in others is very different. If we can imagine a common hernial sac reaching to the bottom of the *scrotum*, and covered by the *cremaster* muscle, and that the posterior half of the sac covers, and is united with, the *testis*, *epididymis*, spermatic vessels, and *vas deferens*, and that the anterior half of the sac lies loose before all those parts, it will give a perfect idea of the state of the *peritonæum*, and of the *testis* when it comes first down into the *scrotum*. The *testis* therefore in its descent does not fall loose, like the intestine or *epiploon*, into the elongation of the *peritonæum*; but it slides down from the loins, carrying the *peritonæum* with it; and both itself and the *peritonæum* continue to adhere by the cellular membrane to the parts behind them, as they did when in the loins. This is a circumstance which I think may be easily understood; and yet I should suppose that it may not be so very intelligible, because I find students very generally puzzled with it, and imagining that, when the *testis* comes first down, it should be loose all around, like a piece of the gut or *epiploon* in a common *hernia*. The ductility of the *peritonæum*, and its very loose connection by a slight cellular membrane to the *psoas*, and to all the other parts around the *testis*, are circumstances which favour its elongation and descent into the *scrotum* with the *testis*.

It is plain from this description, that the cavity of the bag, or of the elongation of the *peritonæum*, which contains the *testis* in the *scrotum*, must at first communicate with the general cavity of the *abdomen*, by an aperture at the inside of the groin. That aperture has exactly the appearance of a common small hernial sac: the spermatic vessels and *vas deferens* lie immediately behind it, and a probe passes readily through it from the general cavity of the *abdomen* down to the bottom of the *scrotum*. And if this process of the *peritonæum* be laid open through its whole length on the forepart, it will be plainly seen to be a continua-

tion of the *peritonæum*; the *testis* and *epididymis* will be seen at the lower part of it, without their loose coat, the *tunica vaginalis*; and the spermatic vessels, and the *vas deferens* will be seen covered by the posterior part of the bag, in their whole course from the groin to the *testis*.

Thus it is in the human body, when the *testis* is recently come down; and thus it is, and continues to be through life, in every quadruped, which I have examined, where the *testis* is in the *scrotum*; but, in the human body, the communication between the sac and the cavity of the *abdomen* is soon cut off: indeed I believe that the upper part of the sac naturally begins to contract, as soon as the *testis* has passed through the muscles. This opinion is grounded on the following observation. I have seen an instance, where, from the age of the *fœtus* and from every other mark, it was probable that the *testis* was very recently come down, and yet the upper part of the sac was very narrow: I pushed the *testis* upwards, in order to see if it could be returned; the attachments of the *testis* easily admitted of its ascent, and so did the aperture in the tendon of the external oblique muscle; but the orifice and upper end of the sac would not, by any means, admit of the *testis* being pushed quite up into the *abdomen*. However this may be, the upper end of the sac certainly contracts, and is quite closed, in a very short space of time; for it is seldom that any aperture remains in a child born at its full time. The lower part of the sac remains open or loose, even in the human subject, through life, and forms the *tunica testis vaginalis propria*, the common seat of an hydrocele. This contraction and obliteration of the passage seems to be a peculiar operation of nature, depending upon steady and uniform principles, and not the consequence of inflammation, or of any thing that is accidental: and, therefore, if it is not accomplished at the proper time, the difficulty of bringing about an union of the part is much greater; as in children who have had the sac kept open by a turn of the intestine falling down into the *scrotum* immediately after the *testis*. This looks as if nature, from being balked when she was in the humour of doing her work, would not, or could not so easily do it afterwards. I shall readily grant that what has been advanced here as a proof of the doctrine, may be explained upon other principles. This at least is certain, that the closing of the mouth, and of the neck of the sac, is peculiar to the human species; and we must suppose the final cause to be the prevention of ruptures, to which men are so much more liable than beasts, from their erect state of body.

What

What is the immediate cause of the descent of the *testis* from the loins to the *scrotum*? It is evident that it cannot be the compressive force of respiration, because commonly the *testis* is in the *scrotum* before the child has breathed; that is, the effect has been produced before the supposed cause has existed. Is the *testis* pulled down by the *cremaster* muscle? I can hardly suppose that it is; because, if that were the case, I see no reason why it should not take place in the hedge-hog, as well as in other quadrupeds.

Why do the *testes* take their blood-vessels from such distant trunks? Those physiologists, who have puzzled themselves about the solution of this question, have not considered, that in the first formation of the body, the *testes* are situated, not in the *scrotum*, but immediately below the kidneys; and that therefore it was very natural that their blood-vessels should rise in the same manner as those of the kidneys, but a little lower. The great length of the spermatic vessels in the adult body will no doubt occasion a more languid circulation, which, we may suppose, was the intention of nature.

The situation of the *testis* in the *fœtus* may likewise account for the contrary directions of the *epididymis* and of the *vas deferens* in adult bodies, though these two in reality make only one excretory canal. In the *fœtus* the *epididymis* begins at the upper end of the *testis*; and it is natural, considering it as an excretory tube, that it should run downwards. And it is as natural that the rest of the tube, which is called *vas deferens*, should turn inwards at the lower end of the *testis*, because that is its most direct course to the neck of the bladder. Thus we see that in the *fœtus* the excretory duct is always passing downwards. But the *testis* is directed in its descent by the *gubernaculum*: and this is firmly fixed to the lower parts of the *testis* and *epididymis*, and to the beginning of the *vas deferens*, and thence must keep those parts invariable in their situation with respect to one another: and therefore in proportion as the *testis* descends, the *vas deferens* must ascend from the lower end of the *testis*; and it must, from the passage through the abdominal muscles down to the *testis*, run parallel with the spermatic vessels.

The *testis*, its coats, and the spermatic chord, are so often concerned in some of the most important diseases and operations of surgery, particularly in the *bubonocoele* and *hydrocele*, that their structure has been examined and described by the surgeons, as well as by the anatomists, of every age. Yet the descriptions of the clearest and best writers upon

the subject differ so much from one another, and many of them differ so much from what is obvious and demonstrable by dissection, that it would seem difficult to account for such a variety of opinions. The very different state of the parts in the quadruped, and in the human body, no doubt, must have occasioned error and confusion among the writers of more ancient times, when the parts of the human body were described from dissections and observations made principally upon brutes: and the circumstances in the structure of the parts, which are peculiar to the *fœtus*, having been imperfectly understood, we may suppose, has likewise contributed to make perplexity and contradiction among authors.

Baron Haller, in his *Opuscula Pathologica*, has observed that, in infants, sometimes the intestine falls down into the *scrotum* after the *testis*, or along with it, and occasions what he calls the *hernia congenita*. In such a case the hernial sac is formed before the intestine falls down, as that ingenious anatomist has observed. There are besides two very peculiar circumstances in a rupture of this kind; the intestine is always in immediate contact with the *testis*, and there is no *tunica vaginalis propria testis*. The structure of the parts in the *fœtus* explains, in the most satisfactory manner, both those circumstances, however extraordinary they must appear to a man, who is only conversant with the structure of the parts in subjects of a more advanced age: and indeed it is so clear that it needs no illustration. I may observe, however, that the *hernia congenita* may happen, not only by the intestine falling down to the *testis* before the aperture of its sac be closed, but perhaps afterwards: for when the sac has been but recently closed, it seems possible enough that violence may open it again.

It must likewise be obvious to every anatomist, who examines the state of the *testis* in children of different ages, that the mouth and neck only of the sac close up, and that the lower part of the sac remains loose around the *testis*, and makes the *tunica vaginalis propria*. Whence it is plain that this tunic was originally a part of the elongated *peritonæum*: and as that tunic is undoubtedly the seat of the true *hydrocele*, it is also plain that the *hernia congenita* and the true *hydrocele* cannot exist together in the same side of the *scrotum*; for when there is a *hernia congenita*, there is no other cavity than that of the hernial sac; and that cavity communicates with the general cavity of the *abdomen*.

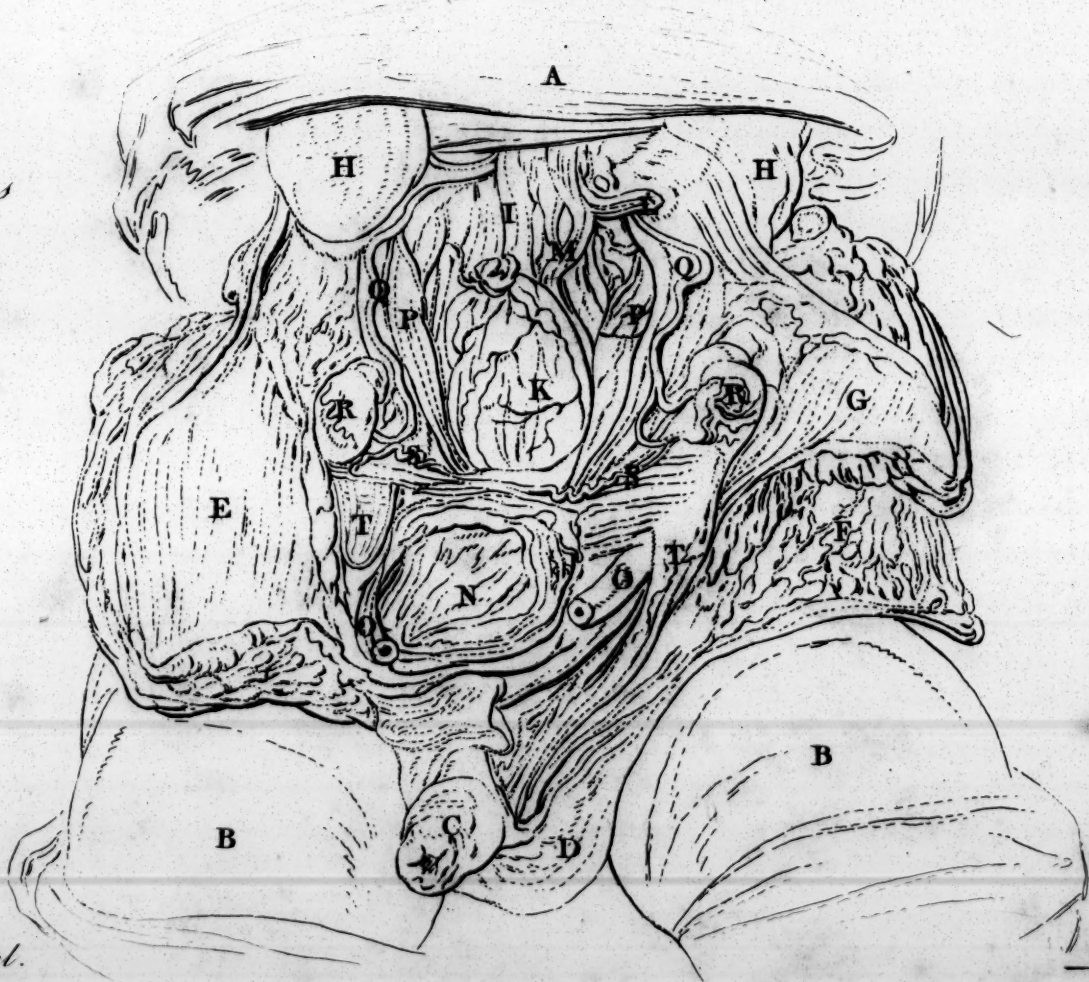
The observations, contained in the two last paragraphs, occurred to my brother upon reading Baron Haller's *Opuscula Pathologica*, and gave rise to



Fig. I.



*Out-lines
of
Fig. I.*



J. V. Riemsdyk del.

P. C. Canot sculp.

to my inquiries upon this subject. That the descriptions which I have given may be better understood I have annexed three figures that were carefully taken from nature.

The first figure represents the *testes* within the *abdomen*, in an abortive *fœtus* of about six months. All the intestines, except the *rectum*, are removed; and the *peritonæum* in most places is left upon the surfaces which it covers, so that the parts have not that sharpness and distinct appearance, which might have been given to them by dissection.

A The upper part of the object, covered with a cloth.

B B The thighs.

C The *penis*.

D The *scrotum*.

E The flap of the integuments, abdominal muscles, and *peritonæum*, turned back over the right *os ilium* to bring the *testis* into view.

F The flap of the skin and cellular membrane of the left side disposed in the same manner.

G The flap of the abdominal muscles and of the *peritonæum* of the left side turned back over the spine of the *os ilium*. The lower part of this flap is cut away, in order to shew the ligament of the *testis* passing down through the ring into the *scrotum*.

H H The lower part of each kidney.

I The projection formed by the lower *vertebræ lumborum*, and by the bifurcation of the *aorta* and *vena cava*.

K The *rectum* filled with *meconium*, and tied at its upper part where the *colon* was cut away.

L That branch of the inferior mesenteric artery which was going to the *colon*.

M The lower branch of the same artery, which went down into the *pelvis* behind the *rectum*.

N The lower part of the bladder, that part of it which is higher than the *ossa pubes* in so young a *fœtus* being cut away.

O O The hypogastric or umbilical arteries cut through, where they were turning up by the sides of the bladder in their way to the navel.

P P The *ureter* of each side passing down before the *psoas* muscle and iliac vessels, in its course to the lower part of the bladder.

Q Q The spermatic arteries running a little serpentine.

R R The *testes* situated before the *psoas* muscles, a little higher than the *inguina*. In this figure the anterior edge of the *testis* is turned a little

little outwards, to shew the spermatic vessels coming forwards to the posterior edge of the *testis*, in the duplicature of the *peritonæum*: which duplicature connects the *testis*, incloses its vessels, and gives it an external smooth coat, much after the same manner as the duplicature of the mesentery connects the intestine, conveys its vessels, and gives it a polished covering.

The beginning of the *epididymis* is seen at the upper end of the *testis*, from which it runs down on the outside (and therefore in this view behind the body) of the *testis*.

S S The *vas deferens* of each side passing across, in a serpentine course, from the extremity of the *epididymis* at the outside of the lower end of the *testis*, and then before the lower part of the *ureter*, in its way to the *vesicula seminalis*.

T T What I have called the *gubernacula* or ligaments of the *testes* in a *fœtus*. On the left side this ligament is intire, so that it is seen going down from the lower end of the *testis*, through the ring of the muscle, into the *scrotum*: but on the right side its upper and forepart is cut away, that the continuity of the *epididymis* and *vas deferens* may be seen; and no more of the ligament is exhibited than what is situated within the cavity of the *abdomen*.

N. B. The lower part of the ligament, as it is seen in the right side of this figure, lies so loose in the passage through the muscles, and is there so loosely covered by the *peritonæum*, that, when the *testis* is pulled up, more of the ligament is seen within the cavity of the *abdomen*, and then the *peritonæum* is made tight and smooth at that place; but, on the contrary, when the *scrotum* is pulled downwards, the lower part of the ligament is dragged some way down through the passage in the muscles, and the loose *peritonæum* is carried along with it; so that then there is a small elongation of that membrane, with an orifice from the cavity of the belly, like the mouth of a small hernial sac, on the forepart of the ligament.

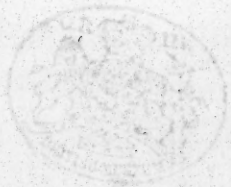
The Second Figure represents nearly the same parts in a *fœtus*, somewhat older, in order to shew the state of the *testes* when they have recently descended from the *abdomen* into the *scrotum*. The small intestines are removed, and the large intestines are left in their natural situation.

A A The liver, in out-lines.

B B The thighs, unfinished.

C The *penis*.

D The



Out-lines of Fig. II.

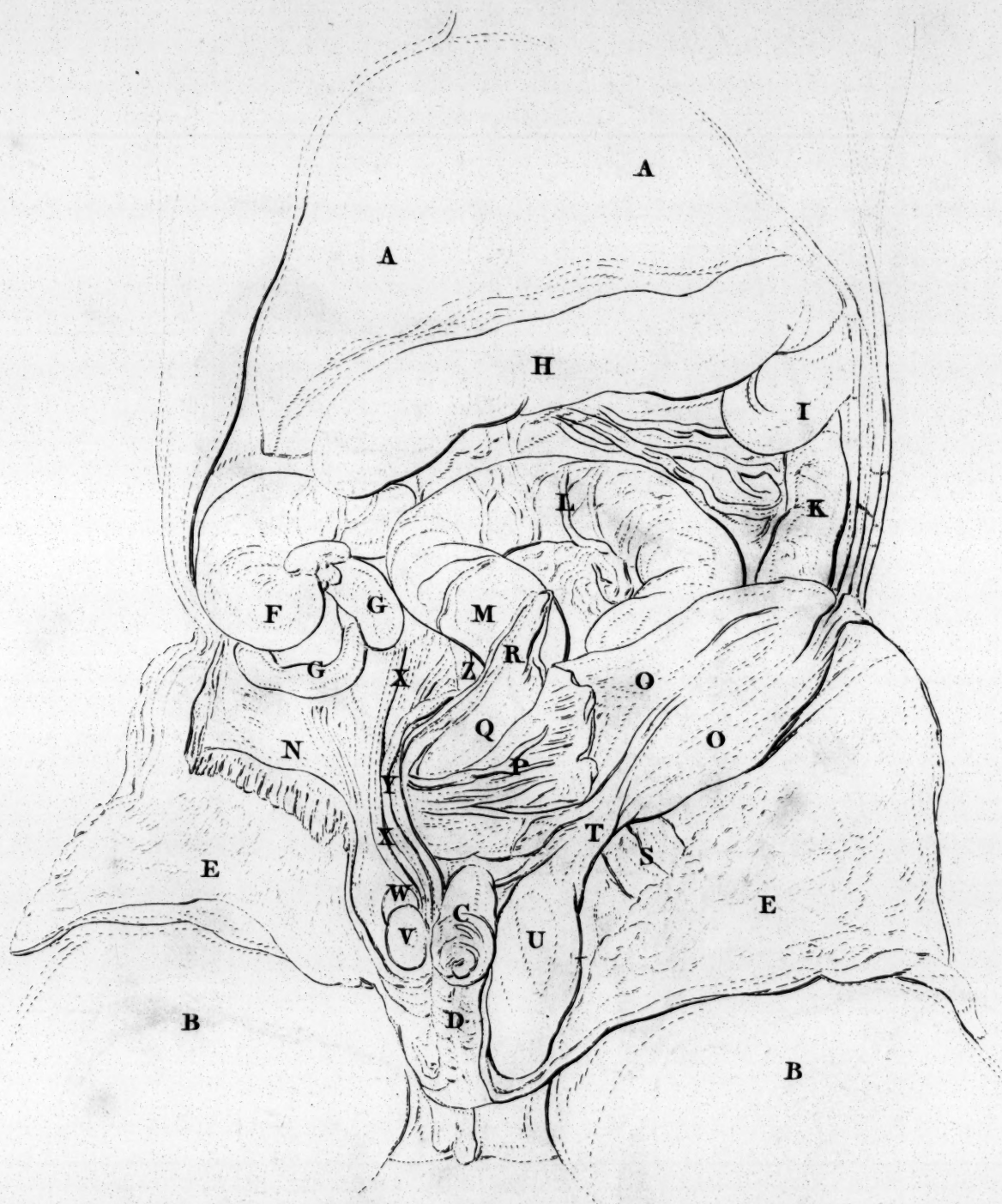
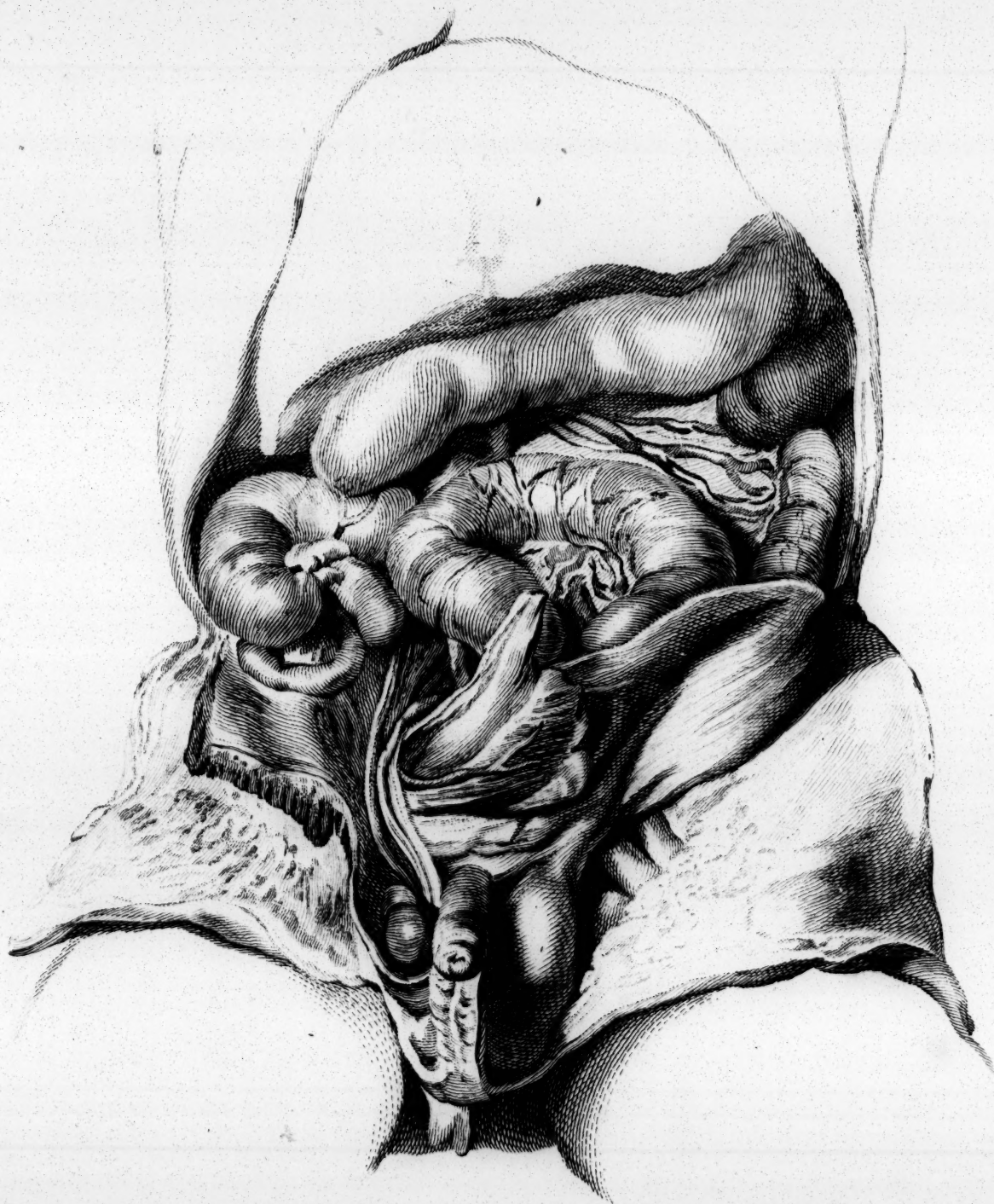


Fig. II.





- D The middle part of the *scrotum*; on each side of which the forepart of the *scrotum* is cut away, that the *testes* may be seen.
- E E The two flaps of the skin and of the cellular membrane dissected off from the lower part of the *abdomen*, and turned down upon the thighs.
- F The *intestinum cæcum*.
- G G The *appendicula cæci vermiformis*.
- H The arch of the *colon*.
- I The turn of the *colon* under the spleen.
- K The *colon* passing down on the outside of the left kidney.
- L The last turn of the *colon*, commonly called its sigmoid flexure, which in adults is seated quite in the cavity of the *pelvis*.
- M The beginning of the *rectum*.
- N Part of the abdominal muscles of the right side, with the smooth investing *peritonæum*, turned back over the spine of the *os ilium*.
- O O The lower part of the *obliquus externus* muscle of the left side.
- P The lower part of the *rectus* muscle on the right side, turned outwards, and towards the left side, so that the epigastric artery is seen going to the inside of that muscle.
- Q The forepart of the bladder.
- R The *urachus*, as it is called.
- S The crural vessels coming into the thigh from behind the *ligamentum Fallopii*.
- T The external appearance of the spermatic rope of the left side.
- U The external appearance of the *testis*, when its *tunica vaginalis*, or process of the *peritonæum*, is a little distended with air or water poured into it from the cavity of the *abdomen*.
- V The right *testis*, brought fully into view by laying open the process of the *peritonæum* in its whole length.
- W The *epididymis* of the same side.
- X X The spermatic vessels.
- Y The *vas deferens*. N. B. The *peritonæum* lies before the spermatic vessels and *vas deferens*, or covers them within the *abdomen*; and its process or elongation covers them in the same manner all the way from the abdominal muscles downwards; so that if the intestine slips down after the *testis* in a *fœtus* it must be placed before the spermatic vessels and *vas deferens*.
- Z The *ureter*.

& The

& The remains of the *gubernaculum* or ligament which bound and conducted the *testis* to the *scrotum* *.

N. B. It is evident that part of the *peritonæum*, which in this figure, is carried down in the form of a hernial sac to a little below the *testis*, lies before the *testis*, *epididymis*, spermatic vessels, and *vas deferens*, and that it covers those parts in the same manner as it covers the abdominal *viscera*, viz. the posterior part of the sac, (supposing the sac to be cut lengthways into two halves) is united with them, and gives them a smooth surface, while the anterior half of the sac lies loose before them, and may be removed to some distance from them, as when the sac is distended with water.

The Third Figure represents the *testes*, &c. in the same subject; all the parts above the *ossa iliûm* being cut away, and the abdominal muscles and the bladder being turned downwards.

A A The thighs, unfinished.

B The *penis*.

C The middle part of the *scrotum*, its lateral parts being removed to shew the *testes*.

D D The skin and cellular membrane of the *abdomen* turned down over the thighs.

E E Part of the abdominal muscles and *peritonæum* turned down at each groin.

F F The *peritonæum* covering the *iliacus internus* muscle of each side.

G The *intestinum rectum* filled with *meconium*.

H The bladder, with the umbilical artery on each side of it, turned a little forwards over the *symphysis* of the *pubes*.

I I The ureters passing over the *iliac* vessels to the *pelvis*.

K The right *testis* exposed, as in Fig. II. V. W. XX. Y.

L The left *testis* inclosed in the process of the *peritonæum*, See Fig. II. U.

M The spermatic vessels of the left side, seen through the *peritonæum* which covers them, in their descent through the abdominal muscles at the groin.

N The left *vas deferens* seen through the *peritonæum*, in its passage from the mouth of the sac to the posterior part of the bladder.

O The mouth or aperture of the process of the *peritonæum*, whereby its mouth or cavity communicates with the general cavity of the belly. This aperture closes up, and the membrane becomes smooth

* The letter-engraver forgot to put the mark (&) of reference into the plate: it should stand where a strait line falling perpendicularly from the letter V would meet a strait line drawn from E to D.

Fig. III.



Out-lines of Fig. III.





smooth at this place, when the *fœtus* grows a little older; unless when the gut falls down after the *testis*, and keeps it open. In that case it makes the mouth of the hernial sac.

P The left epigastric artery branching upon the inside of the *rectus* muscle, which is here turned downwards and outwards. This artery is always situated, as in this figure, on the inside of the mouth of the hernial sac, or passage of the spermatic vessels."

Thus far my brother. Now I resume my narrative-----

In my autumn course of lectures, 1756, (and indeed in every course, which I have read since that time) I demonstrated the principal things contained in my brother's account of the *testes* in the *fœtus*; and I particularly explained that species of rupture in which the intestine is found in contact with the *testis*. This circumstance of the disease, which had puzzled Mr. Sharp and Mr. Cheselden as well as myself, and which even Mr. Pott regarded as a *lusus naturæ*, was now rendered perfectly intelligible. The discovery was become the novelty of the time among students in London, and other inquirers after anatomical improvements; and many gentlemen of my acquaintance desired to see the preparations which my brother had made; and among the rest my friend Mr. Pott did us that honour, one day during that course of lectures. I was not present. My brother shewed him the preparations with great readiness, and explained to him my hypothesis of the contiguity of the intestine and *testis* in some ruptures. Mr. Pott said nothing at that time of an intention to write upon the subject; but, some weeks afterwards, it appeared, by a public advertisement, that he was soon to publish a treatise on that species of rupture. I was much surprised; however, I thought it proper to say but little, till I should see how he treated the subject. The treatise came out in the month of February or March, 1757. It astonished me, if possible, more than Professor Monro's account of the lymphatics had done. It hardly contained one new idea. It was what any of my pupils might have written; (for the cases given in the end, supported only an uncontested fact) and yet neither my brother's name, nor mine was mentioned. It bore strong marks of *second-hand* observation, and of a *time-serving* hurry in the composition. I complained of this at my lectures: every person to whom I mentioned

the subject expressed his surprize: and the authors of the Critical Review made some reflections which could not be pleasing to Mr. Pott, and which, one would have thought, must have brought on some kind of justification.

I hope Mr. Pott can, and, if so, I think he ought to clear up these disagreeable appearances. If he does it in a candid manner, he must allow that I have not wantonly fought a dispute with him: and if it shall appear that I have misunderstood or misinterpreted any part of his conduct towards me, he shall not find me wanting in my endeavours to do him justice.

A P P E N D I X,

C O N T A I N I N G

What was published in the Critical Review, before Dr. Alexander Monro jun. wrote his *Essays Anatomical and Physiological*, &c.

No. I.

In the Critical Review for September 1757, the Reviewers concluded their account of Dr. Monro's Treatise De Venis Lymphaticis Valvulosis, with the following remarks.

HAVING given a brief account of this performance, common justice demands that we should make a few observations; which, perhaps will, in the reader's opinion, invalidate the Doctor's claim to this important discovery: observations, which were communicated to us by a person of probity, who engages to confirm the truth of his assertions by the testimony of above an hundred unexceptionable evidences. In the mean time Dr. Monro will please to observe, that we are altogether neutral in the dispute, and should be sorry to incur the displeasure of a gentleman, for whose extraordinary talents we have a singular veneration.

Doctor Monro says, the discovery he has made with respect to the lymphatics, was owing to experiments ascertained four years ago. Now, Doctor Hunter has read public lectures in anatomy eleven years; and, in every course, made the following observations on the lymphatic veins: That whereas the most generally received opinion was, that they were a continuation of lymphatic arteries; he, on the contrary, believed them to be *the system of absorbing vessels*, and that they began from all the internal and external surfaces of the body. This belief he founded on these reasons: Every body allows, that all the surfaces of the body are bibulous, or provided with absorbing vessels, by which mercury applied to the skin, collections of water in the breast, belly, or in the cellular membrane, are occasionally taken up, conveyed into the circulation, and strained off again by secretion. That the lymphatic veins perform this

office, seems probable, from the following remarks: I cannot inject them as other veins, by filling the arterial system; so that, in all probability, they are not continuations of the arteries. I have sometimes observed in injecting, that they were immediately filled with wax, when the arteries burst, and the wax was effused into the cellular membrane. This looks as if they took their rise from those cells, like the veins in the spongy part of the *penis*. If they were continuations of arteries, why should they be so plentifully provided with valves, which are not found in the other veins of the *viscera*? But, the most striking argument is the analogy between the lymphatics and lacteals. These two systems are, to all appearance, the same in their coats, in their valves, in their manner of ramifying, in their passage through the lymphatic or conglobate glands, and in their termination, viz. in the route of the chyle. As they are perfectly similar, in every other respect, we must suppose them to be so in their origin and use. The lacteals are known to begin from the surface of the intestines, and to be the absorbents of those parts. There is no difference but the name. The same vessels are called *lacteals* in the intestines, and *lymphatics* in the other parts of the body. This doctrine explains the use of valves, in the lymphatics. In other veins, whether large or small, the fluid is supposed to move onwards by an *impetus* received in the arterial system: but, the case is not the same in vessels that suck up a fluid from a surface. These require valves, that every lateral pressure upon them may have the effect of an impulse at the beginning of the canal, in driving the fluid on towards their termination. This doctrine of the lymphatics is farther confirmed by the absorption and progress of the venereal poison. The lacteals were discovered, traced, and their use ascertained, from the circumstance of a manifest and particular colour in their contents, upon some occasions at least. We have not the same advantage, with respect to the lymphatics: but, in them, what we cannot trace with the eye, we find out by the effects of this poison. We know from observation, that this *virus* may be taken in at any particular part of the body, and thence diffuse itself over the whole constitution. We must suppose it absorbed by the same vessels that absorb its antidote mercury, or any thing else that is carried into the mass of blood by absorption. These things being of a more inoffensive nature, pass unobserved; but, this poison, from its irritating and destructive quality, is apt to raise disturbance in its passage, before it reaches far enough to mix with the blood. Hence the lymphatic glands, through which every absorbed liquor must pass, are so often the parts first affected by

by the venereal taint when it is spreading its contagion through the constitution. This is the theory of the venereal *bubo*. If the infection be received in the most common way, the *bubo* happens in the groin, because the lymphatics of the genitals pass through the inguinal glands: but, if the infection be received at the hand, (a case that sometimes occurs) the *bubo*, for the like reason, is formed in the arm-pit: when the disease is communicated by the lips, the glands of the neck inflame and tumify.

This is the very essence of Dr. Monro's treatise; and these observations have been publicly made by Dr. Hunter to his pupils, for the space of eleven years. In the end of the winter, 1755-6, Mr. John Hunter made several injections of the lymphatic glands and veins with quicksilver; and, in the month of May, 1756, Mr. Riemsdyk finished a fine drawing of them, with the receptacle and duct (from a preparation which the doctor still preserves) in presence of many pupils and occasional visitants.

Dr. Reimarus, who attended Dr. Hunter's course in 1755 and 1756, quotes him for this doctrine of the lymphatics, in his thesis published this year at Leyden. Dr. Monro publishes his treatise at Berlin, without mentioning Dr. Hunter's name, though he attended his lectures with Dr. Reimarus; and though Dr. Hunter was particularly full on the subject of lymphatics during that course, on the supposition that his claim to this discovery would be anticipated. Among other things, he observed, that the lymphatics were raised by blowing or pouring mercury into the conglobate glands. We think Dr. Hunter has some reason to complain of this omission, as Dr. Monro seems to have referred to every other author that ever treated on that subject.

No. II.

The Eighth Article of the Critical Review for November, 1757. The Letter was afterwards acknowledged to have been written by Dr. Alexander Monro, senior.

ALTHOUGH the authors of the Critical Review never intended to take cognizance of any production not previously published, they are nevertheless willing to insert the following letter, as an instance of their candour; and this step they are the more inclined to take, as they will have an opportunity to acquit themselves of the imputation of partiality, which this gentleman endeavours to fix upon them. Neuters they

they still profess themselves to be in the dispute between Drs. Hunter and Monro, as far as conviction, and the right they have assumed to themselves to give their opinion of all literary productions, will allow them to be neuters: when they presume to judge in any dispute, they hope the unprejudiced part of mankind will allow them to be unbiassed in their decision.

That they might throw all the lights they could acquire on this unfortunate controversy between two gentlemen of merit, for whom they have all due regard, they no sooner received the following letter, than they had recourse to Dr. Hunter for a real state of the facts, which gave rise to the contest; and though he could not be supposed to answer paragraph by paragraph a paper which he had not seen, he has furnished us with a detail of some incidents and remarks, which in justice to him we shall insert at the end of the letter; and we cannot help thinking that they will not only serve as an answer to the letter, but also terminate the dispute.

To the authors of the Critical Review.

GENTLEMEN,

1. " After giving an account of Dr. Monro junior's treatise *De venis lymphaticis valvulosis*, inserted in your Review of September last, you add some observations and reflections which have been thought injurious to Dr. Monro; but as you declare yourselves altogether neutral in the dispute, you will do him justice by inserting immediately some observations of a friend of Dr. Monro's, before it be too late, to prevent the prejudices which your reflections may have occasioned against him. As he is at present abroad prosecuting his studies, I thought it my duty, as a friend, who knows the facts relating to the present dispute, to send you a fair state of them.

2. " Dr. Monro, in winter 1752-3, attempting to fill the *epididymis* with quicksilver, in the manner directed by his father both in medical essays, vol. 5. art. 20. § 29. and in a manuscript wrote in the year 1747, on the method of dissecting and of making preparations fit for a regular course of anatomy, observed in some of his experiments that the quicksilver returned by the lymphatic vessels without entering the sanguiferous veins, he wondered how or from whence this happened*; he shewed the preparation to several of his acquaintance, and was very importunate with them to give their opinions what this phænomenon

* De vasis lymph. valvul. p. 1.

“ non depended on. Not satisfied with their answers, he made numerous more experiments, and consulted all the books on this subject he could procure. More than four years ago, I, with many others, saw the preparations, which led him to the general doctrine of the lymphatics being a system of absorbents.

3. “ Dr. Monro, having the design of taking the degree of doctor in the university of Edinburgh, early set about composing such a dissertation as the laws of that university require candidates for this degree to publish and defend, and prepared the one *De testibus & semine in variis animalibus*, in which the treatise on the lymphatics was included, but the part relating properly to the *testes* being rather longer than most such inaugural dissertations, he was prevailed on to withhold from the press the part treating particularly of the lymphatics, but allowed Dr. Black of Glasgow, Dr. Reimarus*, and several others of his friends, the perusal of it.

4. “ Before Dr. Reimarus saw it, I read it, and so far as I can remember, the arguments and experiments were the same as are contained in the treatise *De venis valvulosis lymphaticis*, which he published at Berlin in the beginning of the year 1757, and about which the present dispute is.

5. “ In October 1755, Dr. Monro's dissertation *de testibus, &c.* was printed and published at Edinburgh. Some hundreds of copies were given to students, and sent to anatomists in most countries in Europe. ---In this dissertation he promises a treatise on lymphatic vessels in general---he describes and gives figures of the instruments he made use of in his experiments---tells what are his injecting materials, and how the injection is to be made---gives several figures of the lymphatic vessels of the spermatic chord filled with quicksilver---and in short gives the substance of his general system of lymphatics.

6. “ Soon after the publication of this dissertation, Dr. Monro went to London in absolute ignorance of Dr. Hunter's having any particular opinion concerning lymphatics. Immediately upon his arrival he waited on Dr. Hunter, and gave him a copy of his dissertation, attended his lectures, and was surprised when he heard Dr. Hunter teach the doctrine of lymphatics being absorbents.---In summer 1756, he went abroad for his further improvement, and at the intreaty of

“ some

* See Reimarus's thesis, page 3, note g.

“ some friends at Berlin, to whom he shewed his treatise on the lymphatics, he published it there in the beginning of 1757.

7. “ Let every reader judge from this simple narrative of facts, the truth of which can be proved by the testimony of hundreds, to what or to whom Dr. Monro owed his knowledge of the lymphatics being a system of absorbents. Could it be to Dr. Hunter?

8. “ You may perhaps say, that whatever way Dr. Monro had his knowledge, Dr. Hunter has a right to the honour of the discovery, for he has publickly read lectures in anatomy eleven years, in every course of which he made the observations inserted in your Review, which are said to be the essence of Dr. Monro's treatise; and you appeal to Dr. Reimarus's thesis, where Dr. Hunter is quoted for his doctrine of the lymphatics.

9. “ I dare say your observator has given Dr. Hunter's doctrine very full, as it is now, but as you tell us that Dr. Hunter was particularly full on the subject of lymphatics during that course, to wit, in the winter 1755-6, that is, after he had Dr. Monro's dissertation *de testibus*, and had conversed with Dr. Reimarus, I must beg leave to doubt, whether in the preceding years Dr. Hunter was so explicit as your observator represents.

10. “ I shall surprise you in the following assertion, that your observations are neither the essence of Dr. Monro's doctrine, nor indeed a sufficient proof of the general doctrine of the lymphatics being absorbents.

11. “ Dr. Hunter is said to believe the lymphatics to be the system of absorbing vessels, and asserts that every absorbed liquor must pass through the lymphatic glands.---Dr. Monro proves, that the lymphatics are a system of absorbing vessels, but allows that only a portion of absorbed liquors passes into the lymphatics, while the inhalant branches of the sanguiferous veins take also a share of the absorbed liquors*.

12. “ You

* Dr. Hunter not only believed the lymphatics to be the system of absorbing vessels, but gave very sufficient reasons for that belief. Vide Critical Review, No. XX. That the inhalant branches of the sanguiferous veins take also a share of the absorbed liquors, is the old doctrine, which seems to be inconsistent with the discovery made as to the use of the lymphatics. That the lymphatic veins are a system of absorbents, has been proved: that the sanguiferous veins are furnished with inhalant branches for the same purpose, has been supposed: but nature would hardly form two systems for the same operation. Such a supposition is inconsistent with the simplicity, uniformity, and perfection of her works.

12. " You and your observator both have omitted the principal, I had almost said the only convincing proofs which Dr. Monro has brought of the lymphatics being a system of absorbents, to wit, his numerous experiments of throwing quicksilver into different organs; and of the lymphatics being filled when parts are macerated in water, and of their being filled in injecting the excretories of glands, and many more such experiments, as well as morbid cases, and the effects of medicines mentioned every where through his little book.

13. " I should have thought it incumbent on you and your observator to have told us of numerous such experiments done by Dr. Hunter, previous to Dr. Monro's having wrote this treatise on the lymphatics, before you had endeavoured to rob Dr. Monro of any little honour there may be in a discovery of this kind, or before throwing out any indiscreet hints of his having stolen it from Dr. Hunter*.

14. " The arguments, as well as facts, mentioned by your observator in proof of the doctrine, are all taken notice of in books, and were generally known long ago, and therefore if Dr. Hunter in his lectures related no new experiments, nor shewed any preparations that had not been made by Nuck and others, I can see no necessity Dr. Monro was under of taking any notice of Dr. Hunter; he might indeed have mentioned him as an old master, but he could never quote him as one from whom he had learned any thing new relative to the lymphatics. Perhaps at this distance from London I may be doing great injustice to Dr. Hunter, he may have done numerous experiments, and made many preparations before the year 1754, though your observator has omitted them; but as I hear Dr. Hunter intends to publish something on this subject, we shall then know from himself what experiments he had made, and how far he had prosecuted the dissection of the lymphatic vessels before Dr. Monro wrote his treatise.

15. " Why does your observator quote Reimarus for having attended
O " Dr.

* That Dr. Hunter has made numerous experiments, is not to be doubted; but surely we were under no obligation to particularize these experiments, especially as the arguments we borrowed from his lectures, were, in our opinion, conclusive. But, granting he had made no experiments, he first declared that the lymphatics were the system of absorbents: he supported this declaration with solid and satisfactory reasons, and therefore he has a good claim to the discovery. He may plead that those experiments mentioned by Dr. Monro were no other than a superstructure built upon his foundation; and doubtless had reason to expect that Dr. Monro would have taken notice of him in his treatise published at Berlin.

“ Dr. Hunter’s lectures along with Dr. Monro, and for having menti-
 “ oned in his thesis Dr. Hunter’s teaching this doctrine, without telling
 “ us at the same time that in the very same note Dr. Reimarus tells us
 “ that he had read Dr. Monro’s treatise before he came to London ?

16. “ Why does your observator make Dr. Hunter to suppose that his
 “ claim to the discovery would be anticipated, but because Dr. Monro
 “ had already done it in his inaugural dissertation, where he has the fol-
 “ lowing passage: *Hæc inquam, inter alia, argumenta non levia suppedit-*
 “ *tarunt vasa lymphatica valvulosa per totum corpus, venarum absorbentium*
 “ *systema esse, neque, uti vulgo fertur, ab arteriarum surculis emanare. Sed*
 “ *omnia nunc, quæ de hac re disputari possunt, proponere, longe ultra talis*
 “ *dissertationis limites excurreret: & multo aptius de iis, de eorum scilicet*
 “ *origine, fabrica, agendi ratione & usu, si quando per otium licuerit, seor-*
 “ *sim agetur.* Dissert. inaugur. pag. 55, 56.

17. “ After reading this quotation, I suppose our readers will be a
 “ good deal surpris’d at your observator’s having thrown out any hint of
 “ Dr. Monro’s having clandestinely stole any thing from Dr. Hunter,
 “ especially when he is told that a full year and a half elapsed be-
 “ tween his promising the separate treatise on the lymphatics, and his
 “ publishing it; and that Dr. Hunter knew this, and was in the mean
 “ time writing and publishing on other subjects, in which no body could
 “ have anticipated him for want of the drawings with which they are
 “ illustrated.

18. “ What Mr. John and Dr. Hunter have done since the beginning
 “ of the year 1755, is out of the present question; nor do I see what
 “ the design can be of mentioning the preparations made by them in the
 “ year 1756, unless it be to anticipate whatever shall be in the treatise
 “ and figures which Dr. Monro promises at the end of the one *de vasis*
 “ *lymphaticis valvulosis*, and to claim the honour of whatever may be
 “ contained in it. Since Dr. Hunter’s figures were made more than a
 “ year ago, why don’t he prevent his rival by an early publication of a
 “ treatise on the lacteal and lymphatic vessels, and the lymphatic glands,
 “ for Dr. Monro’s has been ready some time, and will probably be soon
 “ published * ?

19. “ Dr.

* No person has a right to ask, why does not Dr. Hunter publish his discoveries ? He may be so engaged in other more necessary avocations, that he cannot spare time sufficient to superintend a publication that requires accuracy and precision: perhaps he postpones publication until he shall have made further progress in his researches, and brought his discoveries

19. " Dr. Hunter keeps himself unnecessarily in a disagreeable situation by the discoveries or improvements published by other people, being claimed by him or his friends as of right pertaining to him. Perpetual disputes with Pott, Haller, Albinus, Monro, or perhaps twenty more, will prove very troublesome, and will at last redound very little to the doctor's honour. Rudbeck appealed to scholars and friends for his having knowledge of the lymphatic vessels before Thomas Bartholin published his description of them, but could not persuade anatomists to take the honour of the discovery from Bartholin.---Van Horn called his scholars to witness, and had the declaration of a very great man, Swammerdam, an eye-witness, in his favour; still however Pecquet is regarded as the discoverer of the receptacle of the chyle and thoracic duct, and De Graaf is esteemed for his treatise on the organs of generation*.

20. " Dr. Hunter has strong calls to avoid these inconveniencies to establish his own honour, and to do service to the community by publishing the improvements and discoveries which he thinks he has made in sound or morbid bodies. He has the example of the greatest men in the anatomical way for this practice. If he neglects to follow it, he may possibly find as few to acknowledge their having profited by him, as he hitherto owns himself to have learned from others, in either his writings or lectures."

No. III.

The Ninth Article of the Critical Review for November, 1757.

Facts relating to the Dispute between Dr. Hunter and Dr. Monro.

ABOUT the beginning of November 1752, in presence of Mr. Galhie and some others, I injected the *vas deferens* in the human body with mercury, and by that method filled the whole *epididymis*, and the tubes.

O 2

coveries to perfection, being loth to follow the examples of those writers on anatomy, who from an eager desire of seeing themselves in print, have rashly ushered into the world productions that were imperfect and erroneous.

* If Rudbeck and Van Horn were defrauded of the discoveries they had made, they certainly were injured, and had reason to complain. But a piece of injustice in one instance will never warrant iniquity in another: these may be precedents, but not a justification of any future fraud or plagiarism. Dr. Hunter never had any disputes with Haller or Albinus: those he has had with other authors have been his misfortune, not his fault. a man who finds himself injured, will naturally complain.

tubes that come out from the body of the *testis* to form it; and observed, in this operation, that the mercury continued to run, and the body of the *testis* to become gradually more turgid and heavy for some time, after the external parts were completely filled.

I shewed this preparation next night at my public lecture, said that I believed we should find the internal *tubuli* likewise filled, but that I would not venture to open it, till I had got another, lest I should spoil what was already a valuable preparation; and desired my brother to lose no opportunity of making the trial.

This was communicated as a piece of anatomical news to Dr. Donald Monro, then at Edinburgh, by a letter from Dr. Garrow, physician at Barnet, some time in the same month.

In some such time as a week or fortnight after this first public demonstration, my brother made the trial, and succeeded. He shewed me the *testis* opened, and the tabular internal substance very generally filled with mercury. This preparation, which I still preserve, I shewed at my public lecture that very evening, with marks of being pleased with the discovery. In the next course of lectures, viz. Feb. &c. 1752, and in every course since that time, I have shewn the same, and some other preparations of the same kind; and always gave the history of the discovery, to avoid taking that share of it from my brother which belonged to him.

Dr. Alexander Monro jun. printed the same discovery first in the Edinburgh essays, vol. 1. pag. 396, in 1754, and then more fully in his inaugural thesis in October 1755, without taking any notice of what I had done upon the same subject. Upon inquiry I found that he had injected the internal *tubuli* in the latter end of January, or in February 1752; that is, two or three months after I had published it at my lectures, and after Dr. Garrow's letter above-mentioned to Dr. Donald Monro his brother. This is attested by Dr. Donne, who was, that winter, a student at Edinburgh, and a companion in anatomical studies with Dr. Alexander Monro jun.

Here you will observe it admits of no dispute that I, or my brother, was the first who injected, and published the injection of the *tubuli testis*, and that Dr. Alexander Monro jun. was the first who printed it,

I wish it were possible to prove this negative, that he did not learn it of me at second hand: I wish it for his sake, because it would clear him, and take nothing from me. But considering that letter of Dr. Garrow, and the constant intercourse between the schools of anatomy at London
and

and Edinburgh, the presumption must always be against him. Such are the facts concerning the first subject in dispute between us : with regard to the lymphatics, the facts are as follow.

Ever since I first read anatomical lectures, in 1746, among other things a little out of the common way of thinking, I have advanced the doctrine of the lymphatics being the system of absorbing vessels, and have supported my opinion by such arguments and experiments as are mentioned in your Review of last September. This appears by the MS. *syllabus* of my lectures, which I have used in public from the beginning, by many MSS. of my lectures in the hands of those who have studied with me, and by the general testimony of those who have done me that honour. I have many vouchers in my possession from gentlemen who have attended my lectures, and I appeal particularly to the following gentlemen, who are all professors or readers of anatomy now living. Dr. Collignon, professor of anatomy at Cambridge; Dr. Smith, reader of anatomy at Oxford; Mr. Hamilton, professor of anatomy at Glasgow; Mr. Cleghorne, reader of anatomy at Dublin; Mr. Watson, reader of anatomy in London; Mr. Galhie of London, and demonstrator or dissector for the professor of Cambridge. So that the fact of my having taught this doctrine, and supported it by such arguments, for a number of years at my public lectures, cannot, I think, admit of a dispute.

Dr. Alexander Monro jun. advanced this doctrine in a general way, in 1755, in his *thesis* above-mentioned, and hinted an intention of treating it more fully upon some future occasion: immediately after which he came to London, and did me the honour of attending my course; and I hope I shewed him the respect that was due to his own merit, and to the son of one of my first masters in anatomy, who has done honour to his country as a professor of the art.

Here again you will observe there can be no dispute that I was the first who published this doctrine about the lymphatics, and that Dr. Alexander Monro jun. was the first who printed it.

He had been unfortunately exposed to the suspicion of plagiarism in his first setting out as an author; and when I saw by his *thesis* that he was opening another field, where he would be in as great danger of being suspected or censured, I took the first opportunity I could, with decency, of putting him upon his guard, by delivering this doctrine fully at my lecture in his presence, and by adding that I had done so in every course of lectures since the beginning.

That

That students of Edinburgh have many opportunities every winter of knowing what passes in London, is obvious, because every winter some of them go to study at the one place, after having studied at the other.

That the substance of what is new in Dr. Monro's treatise on the lymphatics, is the very doctrine I have taught, every impartial man who understands the subject will allow.

That two persons engaged in the same studies should light on the same discovery, is no ways improbable; but that they should support it by a number of arguments and experiments intirely the same, though it be possible, is surely so improbable, that I could wish Dr. Monro had for his own sake mentioned me in a marginal note.

Jermyn-Street,
Nov. 23, 1757.

WILLIAM HUNTER.

No. IV.

The Eighth Article of the Critical Review for December, 1757.

A Letter from Dr. Donald Monro, to the Authors of the Critical Review.*

GENTLEMEN,

YOU'LL excuse the freedom I take of sending you this, and insisting upon its being inserted in the next number of your Review, without either notes or commentary, since in your last you put in a letter from Dr. Hunter, in which I am mentioned as an evidence of my brother's having stole the discovery of the seminal vessels of the testicle from him, Dr. Hunter, by alledging that I had received a letter from Dr. Garrow of Barnet, informing me of Dr. Hunter's having done it long before my brother; and I think myself obliged, in justice to a brother, to let the world know the true state of those facts. But although I exclude you from making any notes or commentaries of your own, yet I allow you to send

* Though this gentleman has precluded us from the privilege of making notes upon his letter, we cannot help taking notice of the cavalier manner in which we are treated. *He insists, and he excludes, and he allows,* as if fate had subjected the authors of the Critical Review, to the sovereign authority of Dr. Donald Monro. Now we must take the liberty to tell him, that our compliance with his demand is not owing to any regard we have for his injunctions, nor to any obligations we are under with the public, but to a sincere and earnest desire of doing justice, which, in this case, has prompted us to deviate from our plan, so far as to take notice of a dispute that never was printed.

send immediately a copy of this to Dr. Hunter, and to insert after it whatever answer he sends you, with his name put to it. (A.)

Doctor Garrow of Barnet, in December 1752, wrote to me that Dr. Hunter had thrown in quicksilver into the *vas deferens*, that he saw it in the *epididymis*, but did not chuse to cut the preparation to see where the quicksilver had gone till he had made another such. In answer to this, I wrote to Dr. Garrow on the 14th of December (by the return of the post) that Dr. Hunter's preparation of the testicle was a common one, and that he would get the quicksilver to go no further than the *epididymis*. This answer Dr. Garrow carried to Dr. Hunter, and asked him if he knew whether the quicksilver had penetrated any further than the *epididymis*, for that he intended soon to write to Dr. Donald Monro, and would acquaint him with it if he had. To which question Dr. Hunter made no answer, but begged of Dr. Garrow that he would write no more to Dr. Donald Monro; which request Dr. Garrow complied with, and I never received after this, while at Edinburgh, any letter from Dr. Garrow, or from any other person, in which Dr. Hunter was mentioned, nor heard of Dr. Hunter's having injected the feminal tubes of the testicles till months after it was done by my brother; and by having received no answer from Dr. Garrow, I was convinced that Dr. Hunter had got the quicksilver to go no further than the *epididymis*, and therefore never mentioned Dr. Garrow's letter to my brother, as it contained nothing new nor more than had been published the year before by Dr. Haller in the 494th number of the Philosophical Transactions; nor did my brother know that ever such a letter had been wrote till he came to London in November 1755, when he heard of Dr. Hunter's having mentioned it in his lecture (B.)

It may be asked why did I write to Dr. Garrow that Dr. Hunter's preparation was a common one. My reasons for it were these (C)

In medical essays, vol. 5. art. 20. sec. 29, my father has the following passage,—“ It has been doubted whether the *vas deferens* and *epididymis* were continued tubes. To be satisfied in this, cut the *vas deferens* through where it lies on the inside of the *vesicula seminalis*, and take it and the testicle away from the body; press the *epididymis* from its larger towards its smaller extremity, and from that to the cut end of the *vas deferens*, till you have squeezed out all the liquor you can, taking care by squeezing with moist fingers not to let these parts dry too much in doing this; then put a long pipe into the *vas deferens*,
“ and

“ and through it pour quicksilver; the weight of such a high column of
 “ mercury, assisted by your fingers pressing from time to time towards
 “ the testicle, will make the quicksilver go forward in the tortuous ca-
 “ nal about half the body of the *epididymis*, beyond which I never could
 “ make it pass, being, I suppose, stopped there by the liquor of which
 “ the canals were full.”—And in a manuscript wrote in the year 1747,
 on the method of dissecting and of making anatomical preparations, he
 orders this same preparation to be made; and as he suspected that the
 quicksilver might be made to pass further than he had got it, he often
 desired me, as well as Mr. John Campbell and Dr. Thomas Frazer, who
 then studied under him, to attempt pouring in quicksilver into the *epi-
 didymis*; which I did, but not succeeding after a few trials better than
 my father had done, I attempted it no more (D.)

In the year 1751, I studied under Dr. Haller at Gottengen, where I
 found that he had been attempting to make this same preparation, and
 that he had succeeded better than my father or I, and had got the
 quicksilver to pass quite through the *epididymis* into the beginning of the
 femoral vessels of the testicle, but could not get it to go further (E.)

In May 1752, when I came to London, I saw number 494 of the
 Philosophical Transactions, which had been published in 1751, and
 found that Dr. Haller had given both an account and figures of the
epididymis and femoral vessels of the testicle prosecuted by dissection,
 much further than those can be seen before the testicle is cut (F.)

After reading this narrative of facts, I fancy Dr. Hunter himself, and
 every other person, will be convinced that I could have no knowledge of
 Dr. Hunter's having injected the femoral tubes of the testicle from Dr.
 Garrow's letter, and I believe that to this day Dr. Hunter himself does
 not know whether these vessels are injected in that testicle of which Dr.
 Garrow wrote to me, for I am told that he never opened it, but still
 keeps it whole; and therefore I do affirm, both from the experiments
 of others and several I have since made myself, that nobody can even
 now say that the femoral vessels of that testicle are filled with the quick-
 silver: it is as probable that the quicksilver is extravasated within the
tunica albuginea before it went so far as it did in the testicle, of which
 Dr. Haller has given a figure in number 494, of the Philosophical
 Transactions. (G.)

Having thus shewn what grounds Dr. Hunter has for alledging that his
 discoveries were stole, I shall next give some account of my brother's
 injecting

injecting those vessels, which I believe will convince every impartial reader that it was not owing to any hint from Dr. Hunter, but to his father's orders and instructions that he attempted it (H.)

My father finding upon my return to Edinburgh in October 1752, that I had not made all the anatomical preparations that were wanting, desired my brother to make as many as he could according to the directions given in his manuscript on dissections and anatomical preparations, as he had time that winter by my taking the trouble of dissecting for the public demonstrations; and accordingly, amongst others, he attempted this on the 9th of January 1753, the parts of generation having been demonstrated to the pupils on the 8th. Upon finding that the quicksilver had gone much further than he expected, he cut open the testicle that night, and Mr. Donne, Dr. d'Urban, and a great many more saw it next day with the seminal tubes filled, as represented in the figures in the Edinburgh physical essays, and in his inaugural dissertation.—At this time neither I, nor any other person I know of, had received any intelligence at Edinburgh of Dr. Hunter's having succeeded in filling these tubes with quicksilver, nor do I know to this day that Dr. Hunter did it before that time; for the filling the *epididymis* I count as nothing, my father and Dr. Haller had done it before him, and he can only be said to have injected the seminal tubes, or to have known any thing about them after having cut open a testicle where they were filled with quicksilver. *Quæritur*, on what day, or in what week, did Dr. Hunter demonstrate them? It must have been some time after the 19th of December; or why did he not answer Dr. Garrow's question? (I.)

What Dr. Hunter means by saying he published this discovery before my brother, is what I do not comprehend; if he means by publishing, shewing it in his private colleges, he puts a meaning upon the word *publishing* different from what I understand by it, for I dissected five years for my father from 1745 to 1750, and had in the winter constantly numbers of the pupils about me, and was intimate with many of them, especially of the English young gentlemen, yet while I remained at Edinburgh, I never could know one thing Dr. Hunter was doing; and since I have been in London, I have not heard more, except some few things, relative to the present dispute, and what I heard accidentally from the Doctor himself in private conversation (K.)

Dr. Hunter alledges, that from the intercourse there is between the schools of anatomy at London and at Edinburgh, that my brother must certainly have known of and stole the discovery from him; but what-

P

ever

ever stress he may lay upon this, I declare upon my honour, that I never knew, and to the best of my knowledge no other person then at Edinburgh knew, of Dr. Hunter's having filled the feminal tubes with quicksilver; and I hereby beg it as a favour, that if any gentlemen wrote to me, or to my father, or to my brother, or acquainted any of us before the 9th of January 1753, with Dr. Hunter's having filled the feminal tubes of the testicle, that such gentlemen would let Dr. Hunter know it by a letter, and if Dr. Hunter can produce one such well-vouched evidence, Dr. Monro will freely give up all claim to the honour of the discovery, but till that time he must always look upon himself as the first who injected and published figures of these feminal tubes of the testicle (L.)

I hope I have fully satisfied Dr. Hunter's most ardent wishes, and proved that Dr. Monro jun. did not learn *at second-hand from him* any thing about these vessels of the testicle; and for the truth of what is here inserted, I refer him to Dr. Garrow of Barnet, to Mr. Donne, who now lodges in his brother's house, and to Dr. d'Urban of Richmond, and to every gentleman who attended the anatomical lectures at Edinburgh in winter 1752-3 (M)

I don't see how any gentleman can be censured for plagiarism by publishing any thing new he discovers by his own industry in the arts and sciences, even though a hundred, nay ten thousand, should have known it before him; if they neglect publishing, and letting the world know it, it is their fault, and none of his.—In the present case, Dr. Alexander Monro jun. injected the feminal tubes with quicksilver the 9th of January 1753; in April or May drawings were made from them, and a copy of them sent in a few weeks after to Dr. Peter Shaw, physician to his majesty, which Dr. Hunter saw soon after their arrival at London. Dr. Monro did not publish either figures, or any account of them, till Autumn 1754; nor did he know that Dr. Hunter, or any other person, disputed the honour of the discovery with him till November 1755, that he came to London, and attended Dr. Hunter's lectures. (N.)

What Dr. Hunter means by saying, *that by my brother's thesis, he saw he was opening another field* (meaning the lymphatics) *where he would be in as great a danger of being suspected or censured*, is what I do not understand. I thought the human body had been a field open to all physicians, and more particularly to those who made anatomy their profession. My brother had of himself started that subject, and
perhaps

perhaps had carried it a much greater length than Dr. Hunter, before he came to London, or knew of Dr. Hunter's having any opinion about those vessels; but I shall not say more of this at present, but refer our readers to the letter inserted in your Review for December 1, 1757, which was wrote by one who knows every step my brother took in prosecuting those vessels, much better than I do (O.)

I am sorry to have been obliged to appear in this dispute between a brother and a friend; I had declared myself neutral, and was resolved never to have appeared in it at all; but since, contrary to my inclination, my name has been brought in, and I appealed to as a witness by Dr. Hunter, I hope he will excuse this, nor think I have done him any injury by doing justice to a brother who is not present to answer for himself.

London,
December 7, 1757.

DONALD MONRO.

P. S. Never imagining that there would be any dispute about this preparation, I did not preserve Dr. Garrow's original letter; but in the beginning of September last, when I heard of Dr. Hunter's mentioning it as a proof of my having early intelligence of his having injected the feminal tubes of the testicle, I wrote to Dr. Garrow, and desired him to let me know by a letter how far he had ever wrote to me on that subject; to which I had the following answer:

DEAR MONRO,

'Tis impossible for me to charge my memory with the express words in my letter to you, with respect to Dr. Hunter's preparation of the testicle. I searched long for a copy, but in vain; however, I found your answer (contrary to my expectations) the words of which are, "*Mr. Hunter's preparation of the testicle, is a common one; he will get the quicksilver to go no further than the epididymis.*"

To the best of my remembrance my words were, That Mr. Hunter had injected the *vas deferens*, that the quicksilver was seen in the *epididymis*, that he believed it had penetrated further, but did not care to cut the preparation till he had made another such.

I cannot be positive to the above passage, but declare upon my word and honour that I never after wrote to you on that subject.

Barnet,
Sept. 11, 1757.

W. GARROW.

Having wrote to him since to know the date of my letter, which he still preserves, I had the following answer :

DEAR SIR,

The Date of your letter to me from Edinburgh, is December 14, 1752; that of mine to you I do not remember, having no duplicate; but think it must have been a week or ten days before.

Barnet,
Dec. 4, 1757.

I am yours, &c.

W. GARROW.

One day about a month ago, I met the doctor in town, and asked him why he did not answer my letter from Edinburgh? when he told me, that upon shewing it to Dr. Hunter, he desired him to write me no more upon that subject.

No. V.

The Ninth Article of the Critical Review for December, 1757.

To the Authors of the Critical Review.

GENTLEMEN,

IN your last Review I gave an account of the facts, so far as they had then come to my knowledge, relating to a dispute between Dr. Alexander Monroe, jun. and myself. This I did with such regard to justice and truth, that I did not think I should have had occasion to give you or the public any further trouble. But the paper which you have sent me from Dr. Donald Monroe, lays me under the necessity of making some further remarks. He honours me with the name of friend, yet keeps very clear of the appearance of partiality to me. He assumes the character of an evidence, who is sorry to be called upon; yet takes the cause upon himself, says more than there was occasion for, and more than his cause will bear. Through the whole paper he seems to be in a mistake, as to his two material points. First, he seems to have thought that an evidence to an improbable fact will be more readily believed if he shews warmth and prejudice to that side of the question, and adopts the cause as his own. Secondly, he seems to treat me, as if I had wished to convict his brother of plagiarism. The public will tell him, the first was ill judged; and, I do assure him, the last was very far from being true. From the beginning of the dispute I proposed
doing

doing myself justice, securing to myself what I knew was, and what I could prove to be my own: but was very far from desiring to fix dishonour upon his brother; which will be the more readily believed, as it is plain I could reap no advantage from it.

Allow me now to proceed to the particular remarks, which I shall note with letters of reference.

(A.) I could wish to make some observations on the following paragraph in his introduction, if I were sure I understood it, *viz. since in your last you put in a letter from Dr. Hunter, in which I am mentioned as an evidence of my brother's having stole the discovery of the seminal vessels of the testicle from him Dr. Hunter, by alledging that I had received a letter from Dr. Garrow, of Barnet, informing me of Dr. Hunter's having done it long before my brother.* If the reader would take the trouble of looking into my letter in the last Review, he would see that I did not mention Dr. Donald Monro, as an evidence of his having stole; I used no such ungentleman-like expression; nor did I lay claim to *the discovery of the seminal vessels of the testis*: I knew they had been discovered long before I had existence; nor did I say, that Dr. Garrow's letter informed him of Dr. Hunter's having done it (i. e. stole) long before, &c. I must own, however, that I perhaps mistake the meaning of this passage. Let me see again---*evidence of having stole---the seminal vessels---from him Dr. Hunter---alleging---a letter---informing---of Dr. Hunter's having done it long before my brother.* After all, I certainly do not understand it: for it does not inform me which of us stole, and which of us lost the seminal vessels.

(B.) In the second paragraph, he gives an account of Dr. Garrow's letter, and (I am almost ashamed to take notice of it) leaves out the principal part of the information. If the reader will turn to the account which Dr. Garrow has given of his own letter, he will see these words, *that he (Dr. Hunter) believed it had penetrated further, viz. than the epididymis.* Now, why was this material passage suppressed in the beginning of *a true state of those facts*?

He says, Dr. Garrow's letter was written in December; I had said *some time in November.* Now it seems it was written in the very beginning of December. The reader, who makes himself master of the subject, will easily see that it does not affect the argument, whether we put it in November or December; as the intelligence it contained must have arrived before the 9th of January, when Dr. Alexander Monro jun. first injected the *testis* with mercury.

I wish

I wish Dr. D. Monro had not been so particular in his account of my conversation with Dr. Garrow, as he was not present. The conversation was very short, and not greatly in his favour; but it was a private conversation.

The reason he gives for not telling his brother of Dr. Garrow's letter, shews consummate prudence.

(C.) His words were, as he now publishes them from Dr. Garrow, *Mr. Hunter's preparation of the testicle is a common one; he will get the quicksilver to go no further than the epididymis.* Now let us hear his reasons for saying so.

(D.) The first is a curious one. Let us abridge it, that we may see its full force, thus: My father had told us in print that he could never make mercury pass further than half-way through the body of the *epididymis*; and informed me by a MS. that he suspected the quicksilver might be made to pass further than he had got it, and often desired me and Mr. -----, and Dr. -----, to attempt it; which I did, without succeeding better than my father, and attempted it no more. *Ergo*, When I was told that Dr. Hunter had done it, I said it was a common preparation.

(E.) His next argument runs thus: Dr. Haller indeed had succeeded better than my father, or I; but had not filled the *tubuli testis* with quicksilver. *Ergo*.

(F.) His last argument is in itself very short. Dr. Haller had traced these vessels within the coat of the *testis* by dissection. *Ergo*, Dr. Hunter's preparation was common, and he could not fill them with mercury.

(G.) Dr. Monro received information at first that I had filled the *tubuli* as far as could be known before the *testis* was cut open, and that I believed them to be filled within the *testis*, but that I would not venture to open the preparation till I had got another of the same kind. Ought he then to have treated this preparation as a common one, and to have taken upon him to say, that I would get the quicksilver to go no further than the *epididymis*? Does it not appear the most natural supposition, when such a letter was received in the beginning of the winter, while the father and his two sons were setting out jointly in the anatomical business of the season, that he should tell his father of it; as neither he nor his father had ever been able to do it; and tell his brother of it, whose employment that winter was to be the making such preparations as they were deficient in? And, does not this supposition agree very well with what happened afterwards? his brother made the experiment
upon

upon the very first subject, *viz.* January 9th. He succeeded; did what Dr. D. Monro had been told that I believed I had done. It was published immediately as a discovery, at his father's lectures; and afterwards in print, when he (at least) might have known that I had demonstrated the same thing at my public lectures in the month of November, or December, preceding that 9th of January. Was not it natural for me to be dissatisfied with all this?

What wild dispute does the doctor introduce here, about that *identical* first *testis*? have not I said in the state of the facts published in the last Review, that I shewed the *tubuli* filled in another, which was injected by my brother at my desire?

(H.) Some readers perhaps may say here, we are already convinced how it was.

(I.) I allow it, because I believe it, to have been on the 9th of January. In my narrative, indeed, I said, *upon enquiry I found that he had injected the internal tubuli in the latter end of January, or in February, 1752-3.* Dr. Donald Monro had referred me to Mr. Donne for this information, who gave it me in presence of some gentlemen from the best recollection he could make, in the following words, *that it could not be sooner than the middle of January:* but he allows, with great candour, that he might be mistaken in eight or ten days, after so many years, as he had nothing but his memory to direct him; and the reader will be sensible that the 9th of January is the same thing to my argument as the 9th of February.

Nor do I know, says the doctor, *to this day, that Dr. Hunter did it before that time.* Does he assert this because it was my brother who injected the second *testis*? or does he doubt my veracity in asserting that I shewed the internal *tubuli* injected in my autumn course of lectures, 1752? Was not this a public transaction before a great number of witnesses now living?—*For the filling the epididymis I count as nothing; my father and Dr. Haller had done it before him.* In answer to this I shall refer the reader to his father's own words, quoted by him as above, from the Medical Essays, which expressly say he never could do it.——*He can only be said to have injected, &c.* I had told him that my brother injected and cut open the *testis*, and that I shewed it in my autumn course of lectures, in 1752. I took no note of the day. He knows, I presume, that my course begins in October, and ends in December. Let it be any day in these three months that he pleases.

---- Or why did he not answer Doctor Garrow's question? I treated his
answer

answer to Dr. Garrow as I then thought it deserved, and therefore desired Dr. Garrow to drop the subject. I am sorry to have had occasion to put Dr. Garrow's name so often in print. I must at least do him the justice to declare, that it is not meant disrespectfully, and that so far as I know, he has behaved in this dispute with honour and integrity.

(K.) *What Dr. Hunter means, &c.* By *publishing* I only meant *making publicly known*: that is, by my lectures, which are honoured with students from all parts of Great Britain. The institution indeed is private, but if I have not been flattered, its influence has been more extensive.

----*Yet while I remained at Edinburgh I never could know one thing Dr. Hunter was doing.* What shall we say to this, after he has told us that he received a letter from Dr. Garrow, telling him what I had done, and was doing about the *testis*?

(L.) *Dr. Hunter alledges---That my brother must certainly have known of and stole the discovery from him.* I should not have alledged that a gentleman *must certainly have stole*, without being able to prove it. I had no *certain knowledge* in this case, but such presumption, or grounds of suspicion, as the reader has been made acquainted with. I gave the history of the dispute about the *testis*, and lymphatics first at my lectures, and then printed it in your last month's Review, (being invited so to do) in order to secure to myself what is my own. At my lectures I communicate every thing freely for the benefit of students. For some time I have been too much engaged to have leisure for printing my observations: many of them are still crude, and I am flattering myself that I shall be able to bring them, perhaps, to some perfection. I am vexed when I see them printed by other people; especially if there be reason to suspect that I have not been fairly treated. Now let the reader put himself in my place, and say, whether the history which I gave of the facts was not as tender as might be expected.

I declare upon my honour. I have such a respect for *honour*, that I would not have it offended even in thought: and therefore I wish Dr. D. Monro had kept to the only point which concerned him as an evidence in this dispute, and declared that he had not told his brother of Dr. Garrow's letter. Let *honour* appear with as much dignity and force, as possible, when he does appear.

---*But till that time he must always look upon himself as the first who injected and published figures of the tubes.* Many have given figures of these tubes before him: but to avoid wrangling about an expression, and to come to the point: whatever Dr. Alexander Monro jun. may have
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(M) *For the truth of what is here asserted I refer him to, &c.* These gentlemen will probably desire to be excused giving their names as evidences to some things contained in this paper. When we appeal to witnesses, we should specify what they will answer for: else the appeal is mere sound. Neither these gentlemen, nor any body else, can positively say he did not learn it of me at second-hand. The affirmative, from its nature, admits of proof: the negative he may be conscious of; but he never can prove it.

(N) *I don't see how any gentleman can be censured, &c.* However it may appear to Dr. Monro, it is certain the public will not give up their right of censuring when they find cause. They are ever just in giving applause where it is due, and seldom fail to shew their resentment when they are insulted by ill-founded pretensions.

(O) *What Dr. Hunter means by saying——is what I do not understand.* Why write about it then? Especially as it is a dispute between a brother and a friend, where he has not the pretence of speaking as an evidence. As he shews an inclination to be in it, let us try if we can make it intelligible to him by a familiar allusion. A field may be *common*, and yet admit of robbery. Suppose two men have a right to gather sticks in a common; if one of them takes what the other had gathered, he is censured with justice, if he knew it when he took them; and if he did not know it, he is dishonest, and will be censured, if he refuses giving them up, when it is proved that they were become another person's property.

He concludes with referring the reader to the last Critical Review. The reader cannot oblige me more than by considering attentively what is there said upon both sides of the question.

Termyn-Street,
Dec. 22, 1757.

WILLIAM HUNTER.

P. S. Dr. Alexander Monro jun. who was abroad, has been lately in town, and we are therefore bound to believe he has approved of the steps which his brother has taken in his defence.

END OF PART I.

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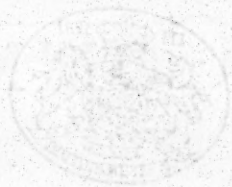
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A
S U P P L E M E N T
To the FIRST PART of
MEDICAL COMMENTARIES.

By Dr. H U N T E R.

First Printed in the Year MDCCLXIV.



THE INTRODUCTION.

PERHAPS it may be as sound philosophy to say, that all the actions of men are directed to some good end, as it is to subscribe to an opinion which has prevailed among naturalists, that, in the works of nature, nothing is absolutely without its use. Literary disputes are disagreeable to the greatest part of mankind; and the disputants are, for the most part, condemned by the world. Yet it is reasonable to think, that even these disputes answer some good purpose. By engaging the passions of men more warmly, they rouse a spirit of emulation, and give a spur to enquiry.

It is remarkable, that there is scarce a considerable character in anatomy, that is not connected with some warm controversy. Anatomists have ever been engaged in contention. And indeed, if a man has not such a degree of enthusiasm, and love of the art, as will make him impatient of unreasonable opposition, and of encroachments upon his discoveries and his reputation, he will hardly become considerable in anatomy, or in any other branch of natural knowledge.

These reflections afford some comfort to me, who unfortunately have been already engaged in two public disputes. I have imitated some of the greatest characters, in what is commonly reckoned their worst part: but I have also endeavoured to be useful; to improve and diffuse the knowledge of anatomy: and surely it will be allowed here, that, if I have not been serviceable to the public in this way, it has not been for want of diligence, or love of the service.

It has likewise been observed of anatomists, that they are all liable to the error of being *severe* on each other in their disputes. Perhaps from being in the habit of examining objects with care and precision, they may be more disgusted with rash assertions, and false reasoning. From the habit of guarding against being deceived by appearances, and of finding out truth, they may be more than ordinarily provoked by any attempt to impose upon them; and for any thing that we know, the passive submission of dead bodies, their common objects, may render them less able to bear contradiction.

But,

But, to be more serious, we must allow that the language and manner of literary war should be adapted to the circumstances. Injuries, disregard of truth, and mean artifices, in one party, will, and ought to be treated with some degree of indignation, by the other. In order, therefore, to judge properly of the manner, we must enter into the cause, and sift it to the bottom, that we may see and feel the situation of the writer; and then, perhaps, what seemed, upon a superficial view, too keen, will appear to be very gentle.

In the ninth chapter of the *Medical Commentaries*, I defended myself against a reproach thrown upon me by professor Monro, senior, of Edinburgh, by giving a clear and concise account of a dispute, which I was unfortunately involved in with Mr. Pott. The account was indeed unfavourable to Mr. Pott; but the circumstances were fairly stated, so far as I could be informed; and I had taken some pains to procure information. I concluded that account by supposing that it was possible that I had misunderstood his conduct towards me; and declared, that if ever I should see reason to think that to have been my case, he should find me ready to do him justice. Here the affair rested till last October, when he published a second edition of his general Treatise on Ruptures. In that he added a chapter on the *Hernia congenita*; and took the opportunity of giving the public his account of our dispute. I read it, and found that we differed very widely in stating the facts upon which the whole dispute between us depends. I remembered the promise I had made, and reasoned thus in my own mind: "Had I been convinced of being in the wrong, I should certainly have excused myself in the best manner I could; but I should as certainly have done justice to Mr. Pott's character, by owning my error, and asking pardon of him, and of the public. Whoever reads his account, and supposes that there are no mistakes in it, must think that it is my duty to do so immediately. Yet, now that I have got all the light which he has given me; when I read over both accounts, and compare them together, I am still conscious that mine is exactly true in every particular; and that in his there are such mistakes and inaccuracies, as could not have been expected from a man of his understanding and abilities, whether one considers him as a surgeon, or as an author. Yet these mistakes happen to be in the great points upon which the dispute turns, and totally change the nature of the case: therefore, justice to the public, as well as to myself, obliges me to clear up the matter."

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I N T R O D U C T I O N.

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The dispute between us owed not its rise to jealousy, private pique, or malevolence, on either side; we lived in common, though not intimate friendship; and so far as I knew, neither of us had the least cause of complaint against the other, till the occasion of this dispute. What I said in my lecture, or in print, was not in the hurry of passion; but with reflection and meaning: And as to the manner of telling his story, I must be so candid as to confess, that if the circumstances had been exactly as he has represented them, I should have thought myself deserving even of a more severe rebuke from him. He has treated me, for the most part, with the language of a gentleman, for which I thank him. I have, indeed, received some *incision* at his hand, but little *butchery*; and I have been so much used to meet with the latter, that I am the more sensible of his lenity.

My purpose in the following pages is to prove the truth of the accusation, which, in my own defence, I brought against Mr. Pott, in the ninth chapter of the Medical Commentaries. To speak my opinion freely upon the whole dispute, I must first declare, that, after having duly considered the defence which he has made in the second edition of his Treatise on Ruptures, published last October, I am so far from repenting of what I said, that I cannot wish to retract one syllable of the accusation. And now I shall enter upon the particulars.

S E C T.

S E C T. I.

Of a supposed plagiarism from Baron Haller.

THE first point in order of time, is, whether Mr. P. borrowed a remarkable paragraph from Baron Haller, and gave it to the world as his own, in the first edition of his general Treatise on Ruptures. He avers (p. 149 of his defence) that he never had seen, read, or heard of, that work of Baron Haller, either in Latin or English, till twelve months at least after his publication. By way of a short introduction to this declaration, he says, "To save the reader's time, and "to cut short this part of the dispute"---Is there any argument in this way of cutting a dispute short? The fact is of too much importance to be cut so short; and I shall, in the sequel, prove, however respectable his veracity may be, that his memory frequently mis-leads him, where one would think it impossible to be mis-led, and betrays him into most disagreeable situations. But surely no man is heard as evidence in his own cause. Evidence must be drawn from the testimony of credible witnesses, (not of parties) or, for want of such testimony, from circumstances.

Let us consider the evidence which he brings. He avers; but does not name one witness. He published a new, a curious, and an useful doctrine of the most common cause of *Herniæ*; and added, "This has always been "my opinion;" which, by the bye, is an officiousness that gives strong suspicion. It looks like a consciousness, that people would immediately say, "This is the opinion which Haller has published within these few "months." Yes, says he, but *it has always been mine*. This, however, is digression. I was saying, he published a new doctrine, which would have done honour to any man of the profession, and said "it had always "been his opinion;" yet now, when that fact is disputed, he cannot, it seems, *for he does not*, bring any one friend, pupil, or acquaintance, to testify, that it was his opinion before the time of Baron Haller's publication. Is it not amazing, that he should not have taught that curious doctrine to his apprentices and pupils? that he should not have mentioned it to me, when we were considering *Herniæ* in a dead body dissected for that purpose? that he should never have mentioned it to such gentlemen as Mess. Hawkins, Sainthill, Nourse, and Webb; to whom, he tells us,

(p.

(p. 145) he communicated his other new idea? If he had, they would not have forgotten so curious an opinion; nor would they have refused him the justice of giving their testimony to truth. "It had always been his opinion;" but, it seems, he never mentioned it to any mortal. Can we account for such *cautious, apprehensive reservedness*, to use his own words, in a gentleman, who is now so very communicative; who writes a book every year for the instruction of the profession, and advertises the contents of all his works, almost every day, in every public paper?

His having brought no testimony must then appear as a strong presumptive proof against him. Let us next see, what degree of probability he has been able to draw from the circumstances of the case. Now let us remember the case; it is allowed by himself to be thus: He published a curious doctrine in surgery; *viz.* that the descent of the *Testes* from the loins into the *Scrotum* is the most common cause of *Herniæ*, as his own, after B. Haller's book, which contained that doctrine, had been even translated into English; yet he insists still, that the doctrine was his own, that it had always been his opinion; and that he had not seen or heard of the Baron's book (which was frequently advertised in our news-papers) till about a year afterwards. That I may do all justice to the arguments brought in proof of this *extraordinary* and *improbable* fact, I shall relate the whole in his own words, and intersperse some remarks, that the reader may the better feel the force of these arguments.

"But (p. 149) setting aside whatever pretension I may have to be believed upon my bare assertion, is it probable that if I had stolen my opinion from the Baron's book, that I should have given so short, so imperfect, and, indeed, so erroneous an account of what he has so fully explained, or, at least, so clearly pointed out?" Whoever will take the trouble of comparing the passages quoted from the Baron and from Mr. Pott †, will see that, if Mr. P. did steal at all, he stole the whole substance; and that no man could venture upon a more literal translation, with any chance of concealing the plagiarism. The name, and other little circumstances, for good reasons, were left out; and B. Haller might perhaps say, *Hic quidem non unam aliquam aut alteram a nobis, sed totam ad se nostram de herniis congenitis observationem transtulit. Atque, ut reliqui fures, earum rerum, quas ceperunt, signa commutant: sic ille, ut sententiis nostris pro suis uteretur, nomina, tanquam*

† Medical Commentaries, part I. p. 73 & 74.

rerum notas mutavit. But, to discuss this point in plain English, surely Mr. P.'s doctrine being *short* and *imperfect*, is no proof that it was not taken from Haller; for Haller's account of it is both *short* and *imperfect*. It was a new observation, and required careful and repeated examinations; therefore Haller, at first, talked as became a true philosopher, with diffidence; and, at last, had hardly a doubt left:—*causa videtur poni—ni fallor—suspicio nondum matura—non sufficiunt experimenta—Hæc omnino merentur considerari a viris gnaris & vericupidis & per experimenta repeti—Hactenus dubius—nullum fere dubium superest.* These expressions shew, that this sketch by the great physiologist, though *short* and *imperfect*, was not struck off at once, and at random, but was the result of observation and patient enquiry; and if he shall be blessed with health and long life (which I most earnestly pray for) he will probably favour us with a more full and perfect account of the matter. Here I cannot help observing how slowly, and with what difficulty, we acquire knowledge by study; yet how quickly and easily it comes by intuition. What Baron Haller took so much pains to find out, was—*always Mr. P.'s opinion.*

Mr. P. also says, that if he had borrowed it from Haller, it was improbable he should have given *so erroneous an account of what he has so fully explained.* This is indeed a specious argument, as it is proposed; but, when examined, it is another very unfortunate one, as it proves what it is brought to disprove. The only error in Mr. Pott's account, that I am aware of, is this; that the *Testis* remains in the *Abdomen* till birth, and is then forced down by breathing, crying, &c. But this very error is in Haller's book; and therefore serves to prove the plagiarism. It was easier to take the whole, than to correct the error. As it was,—*it had always been Mr. P.'s opinion.* The only difference is this: B. Haller published the opinion cautiously, and with hesitation, as it arose in his mind from the examination of a few cases: but Mr. P. took it all without hesitation, and gave his own little bit of a sort of a reason for it; *viz.* It was right the *Testis* should be out of the way of danger till after birth.

We have seen the force of his first argument: It proves what was not intended. He goes on thus: "If I had taken my account of the descent of the *Testes* from thence, why did I not also learn from thence the reason why the Intestine and *Testis* are sometimes found in the same *sacculus*?" Because Baron Haller neither mentioned this case, nor gave any reason for it. What says Mr. P. to this plain answer? I presume he will

will call it *rude*, and *malevolent*, and *unprovoked*; but he must allow that it is a full answer to his second argument, and that hitherto, therefore, he stands justly suspected of plagiarism from B. Haller. He proceeds to urge his second argument thus: "One of these facts was as much the subject of my enquiry, at that time, as the other; and in the *Opuscula Pathologica* (the book alluded to) are both of them satisfactorily accounted for, and made to illustrate each other." The reader will perhaps be amazed when I assure him, that the one fact in dispute, called here *one of these facts*, is neither accounted for satisfactorily, nor unsatisfactorily; nor made to illustrate the other, or to illustrate itself, or to illustrate any thing else; it is not so much as once mentioned.

Let us go to the next argument: he says, "Why should I call the case related by Mr. Sharp a *lusus naturæ*? Why not avail myself thoroughly of the plagiarism, by giving a true solution of the appearance; shewing that it was not a *lusus naturæ*, nor produced by what Mr. Sharp and Dr. Hunter had thought was the cause of it, but by the intestine being pushed into the open *tunica vaginalis*?" Any man who read Aquapendente's Tract on the valves of the veins, might have availed himself thoroughly of it, and explained the circulation of the blood; yet the obvious inference, which had escaped Aquapendente, escaped every body, till Harvey's keen glance caught it. How ridiculous it is in Mr. P. to ask why he did not avail himself of B. Haller's observation, by giving a true solution of the appearance! The question proves only, that it seems to have required a little more thought and attention than he was pleased to give it: which, I presume his acquaintance will not think very strange. "All this is in the same chapter of the same book;" not in the same, nor in any other chapter of the same book. The reader may stare, indeed; but the fact is so. "From this book Dr. Hunter and his brother derived all their knowledge of both these subjects." People naturally judge of others by their own experience of themselves. No; I beg Mr. P.'s pardon: he knows that a good deal of anatomical knowledge is to be got without books or dissections. Let any man, for instance, who knows but the common things, keep a good correspondence with students, or borrow notes taken at lectures, and he may, with very little trouble, become as great a discoverer as a modern *junior professor*, or *senior surgeon*. If the reader will take the trouble of comparing Mr. J. Hunter's account of *both these subjects*, with B. Haller's, he will see what reason Mr. P. could have to assert, with original simplicity, in his defence, that Dr. Hunter and his brother took all their knowledge of both

these facts from this book. “ And this book (if I had read it) must have
 “ informed me of both, as certainly as of one. Is Haller’s account of
 “ one more plain and intelligible than of the other ?” Haller’s account
 of the one is indeed very plain and intelligible; but he has given no
 account at all of the other, neither intelligible nor unintelligible. “ Or
 “ is it likely that I should read only what related to one, and not what
 “ related to the other, when they were not only in the same chapter and
 “ page, but equally parts of the subject I was then enquiring into ?”
 The reader, by this time, may think it very likely, that he read what
 related to the one, and as unlikely, that he should read what related to
 the other, because there is nothing said of the other, either in the same,
 or in any other chapter or page of the book.

“ Indeed, the spirit of criticism, or, more properly, the desire of
 “ finding fault, has in this instance got the better of that artful caution,
 “ with which Dr. Hunter most frequently either expresses or conceals
 “ his sentiments, has carried him beyond the proper mark, and made
 “ him prove too much.” If it is a crime, we must not accuse Mr. Pott
 of artful caution; and we can easily believe, he thinks Dr. Hunter has
 proved too much. But by-standers observe best whether the mark be
 hit or not. “ Since, if I had read the *Opuscula Pathologica* of Haller,
 “ previous to the publication of my general treatise in 1756, I must
 “ have obtained from thence that very information, which the Doctor
 “ says I got from his brother in 1757, at the same time when he is said
 “ to have explained to me the Doctor’s hypothesis; for in that book,
 “ as I have already observed, are contained both the Doctor’s hypothe-
 “ sis, (as he calls it) and Mr. Hunter’s discovery.” The reader must be
 sick of all this over and over; and therefore I will tell him, for the last
 time, that my hypothesis is not contained in that book, nor ever was in
 any book, till Mr. Pott made a pamphlet of it, and took it to himself.
 My hypothesis was, that in some cases of *Herniæ* the intestine must
 lie on the outside of the *tunica vaginalis propria testis*, and in others
 within it. These last were reckoned unaccountable by Mr. Pott, who
 considered them as *accidents*, or *lusus naturæ*; and Haller has made no
 comparison, contrast, or opposition, between the two species. It is
 true indeed, that by reasoning and applying what the Baron says of the
 anatomy of the parts in fœtuses, it is easy to give a solution of Mr. Pott’s
lusus naturæ; and accordingly it struck me when I read Haller, but in
 the way of *inference*; and this I owned in the account which I gave of
 the matter, as freely as Mr. Pott tells what he read in Lagaranne, and
 what

what use he made of it. Mr. Pott must not pretend, that because the doctrine is contained in Haller *by inference*, therefore I did not make the discovery; for if he makes that plea, I shall easily prove, by the same argument, that he had himself made the discovery, when he called it an *accident*, or *lusus naturæ*; that is, when he did not understand it. He called it a *lusus naturæ* in his general treatise in 1756; yet, in that very book, and in the passage which appears so evidently to be taken from B. Haller, he says, "This passage of the *Testis* from the belly into the *Scrotum*, I take to be the principal cause of the ruptures of infants; for the ring, or aperture, being by this means dilated, a portion of caul, or gut, has an opportunity of slipping through, before the aperture has had time to contract itself again." The intelligent reader will see that the discovery is contained in this; because, if the caul, or gut, takes the opportunity of following the *Testis*, before the passage contracts itself, it cannot be otherwise than in contact with the *Testis*, which it follows. Yet he owns now, that he could not then account for the contiguity of those parts in a rupture, and therefore called such a rupture a *lusus naturæ*.

But to return to the subject of plagiarism from B. Haller. Mr. P. goes on thus: "I am very willing to allow that Dr. Hunter might reasonably presume," and the reader surely cannot now doubt, "that I had seen the *Opuscula*; but is such a presumption to be immediately admitted as a proof;" yet you see when it is well examined, it equals demonstration in the conviction which it gives; "or can it be thought sufficient to authorize or vindicate so rude and so unprovoked an attack as he has made on me?" Now, after all, this *rude* and *unprovoked* attack, as he would wish the reader to believe it to have been, was made upon him in the following manner. I was accused by professor Monro, senior, of having a dispute with Mr. P. I *knew* that Mr. P. had taken an observation from me, and assumed the honour of it to himself; therefore my attack was not *unprovoked*: whatever the attack was, it was made on that account. In the introduction to my accusation of him, I had occasion to quote a remarkable passage, which I was then convinced (and now have proved) was taken from B. Haller: yet all that I said of it was this, that *I felt some uneasiness for my friend*. Surely that was *gentle*, not *rude*. I appeal to his friends. But if he insists that it was *rude*, I will cut this point very short, by recantation: I beg his pardon for having said so; and now declare, with great civility, that I feel no uneasiness at all for my old friend. If the reader does, I
must

must applaud his generosity; and can say, with great sincerity, I was once in his situation; and think it very probable he will come to mine, when he knows his friend a little better.

S E C T. II.

The true State of Mr. P.'s Visit to Mr. J. HUNTER.

TO follow the order of time, the next enquiry should be into the account which Mr. Pott has given of the occasion and circumstances of his discovering and ascertaining the nature of the particular species of *Herniæ*, which made the subject of his pamphlet. Here he stands accused of plagiarism from my brother and from myself. One of the most important circumstances of the transaction is a visit which he paid to my brother. I shall begin with that visit, because it is important; because it will serve as a key to other things; and because Mr. P. and I represent it in such different lights: it shall be the test between us, of proper behaviour, of candour, and veracity.

Mr. P. pretends (p. 145) that he called at my house in Covent-Garden with an intention of telling me what he had done; that he learnt nothing from my brother, &c. "He shewed me *one single preparation*," says he; "he did not shew me any other preparation——nor do I remember that the congenial Hernia was once mentioned by either of us during my short visit, notwithstanding the Doctor has said that his brother "shewed me his preparations with great readiness, and explained to me his (the Doctor's) hypothesis of the contiguity of the intestine and testicle." Our conversation turned entirely on the passage of the *Testes* from the belly into the *Scrotum*; and, as far as I could perceive, (for he spake with the most cautious, apprehensive reservedness) our sentiments were alike.

"My papers were at this time finished, and corrected for the press; ——nor did I alter a single syllable in them, in consequence of this visit to Mr. Hunter. But had that gentleman been half so explicit as his brother represents him to have been; had he been so ingenuous as to have told me, that either he or the Doctor had regarded themselves as the discoverers; had he signified that either of them had any intention to say, or to publish any thing about it——I would either have suppressed my book, or have mentioned their names in it.——And as
" to

“ to the honour of the discovery, it would not have given me any concern at all.

“ This is a short and true account of the fact; this is the thing for which I have been traduced in print.----Page 149. The manner in which I attained my knowledge I have already most faithfully related. ----Page 156. But excepting that single circumstance of not having related the short conversation which passed between his brother and me, and from which I did not derive the least degree of information, ---Page 162. When I published my tract on the congenial Rupture, I had no intention to anticipate either of them, or to prevent either of them from enjoying any reputation or honour, which might arise to them from their labours on this, or any other subject: if he (Dr. H.) had said, that he or his brother was then enquiring into that part of the animal œconomy, I should most probably never have prosecuted my enquiries,---as I should have known that the subject was in so able hands: I want no reputation of that sort.”

Now the reader shall judge between us, from positive and unquestionable evidence, which the point in question happens to admit of.

My brother gives me the following account of Mr. Pott's visit.

“ One morning, some time in the autumnal course of lectures 1756, Mr. Pott called upon me in Covent-Garden, and spoke to me of the preparations which I had made relating to the *Testes* and *Herniæ* of children, and expressed a desire of seeing them. I went with him into the preparation-room, and we examined them together; and some gentlemen, who lived with me at that time, were in the room with us, or at least were coming and going, for we were some time together; and after we had examined and talked of these matters, Mr. Pott came into the parlour with me, and sat with me some time longer. I cannot take upon me to say which, or what number, of those preparations were then examined; but to the best of my knowledge, I shewed them all; and I had several at that time. I told him what I had done, and told him the use you (Dr. Hunter) had made of these observations, in explaining the different situations of the intestine in *Herniæ*, viz. Whether it lies in contact with the *Testis*, or on the outside of the *Tunica vaginalis*. I particularly remember that he was then of opinion, that respiration was the cause of the descent of the *Testis*, as he had explained it in his book of Ruptures, which was published some months before; and that I took the liberty
“ of

“ of declaring against that opinion, and told him I had commonly
 “ found them out of the *Abdomen* before the time of birth. Mr. Pott
 “ did not tell me, or give any hint which I understood, that he had
 “ an intention to publish upon the subject.

“ 8th of Oct. 1763.

John Hunter.”

My brother's verbal account at the time, was enough for my satisfaction; because I was as sure of the truth of what he said, as I could have been by the evidence of my own senses. But some time after the Critical Reviewers had taken notice of Mr. P.'s pamphlet, I was told, by a gentlemen of the profession, that Mr. P. had been attacked in company about his discoveries, and that he positively denied having ever seen our preparations. Upon this information, I applied to Mr. Lufcombe, surgeon, of Exeter, who was in my brother's house at the time of Mr. Pott's visit; and I desired my brother to write to Mr. Patch, surgeon, then of Exeter, but now of London, who was likewise present. In answer to my application, Mr. Lufcombe wrote to me as follows:

“ SIR,

“ In the autumnal course of your lectures, which I had the pleasure
 “ to attend, boarding then with your brother, I perfectly remember that
 “ Mr. Pott called on him about the latter end of the course, (which
 “ began Oct. 4th, 1756) and that your brother then demonstrated to
 “ him the situation of the *Testis* in the *Fœtus*; the manner of its passing
 “ down into the *Scrotum*; the species of rupture when in contact with
 “ the *Testis*; and shewed its situation, and explained the manner of
 “ its passage, with your opinion about that rupture; viz. that it was
 “ produced from infancy, being what is called the Congenial Rupture,
 “ which was fully explained in the same course. Returning you my sincere thanks, &c. &c.

“ Exeter, May 5th, 1759.

In Mr. Patch's letter to my brother, which is a long one, upon a variety of subjects, is the following paragraph: “ In answer to your enquiry, if I can recollect being present at the time Mr. Pott saw your preparations, I perfectly remember that Mr. Lufcombe, one morning, I believe in November last, came into my room, and told me that Mr. Pott was in the preparation-room with you; on which I went in,
 “ and

“ and saw you two looking on those preparations of the *Fœtus*, where
 “ the *Testes* are seen descending into the *Scrotum*, and the *sacs* or pro-
 “ cesses of the *Peritonæum*, that are afterwards to become the *Tunicæ*
 “ *Vaginales*. You then told him, you had taken drawings of those parts ;
 “ and that the Doctor, in his lectures of the former winter, had ex-
 “ plained, from these preparations, the manner in which a congenial
 “ rupture is formed ; which I likewise had heard from some of the pupils
 “ who attended him at that time.” In another part of the same letter,
 speaking of the account given in the Critical Review of Mr. Pott's pam-
 phlet, he says, “ I can vouch for the truth of all that is there said, except
 “ the quotation from Mr. Cheselden, and that I do not remember that
 “ the drawings of the parts were shewn at lectures, though Mr. Luf-
 “ combe and I had the pleasure of seeing them among your curious col-
 “ lection of drawings.” This letter is dated, “ Exeter, June,” viz.
June 1758, and signed, “ James Patch.”

Here is such evidence as requires no comment ; it settles the point in question, and renders all argumentation or declamation equally useless ; it is the concurring testimony of two gentlemen of the profession, who understand the subject, who are independent and disinterested ; it proves that I had shewn these preparations, and taught that doctrine of *Hernia* in my public lectures, even before Mr. Pott's first book was published, at which time he owns that he knew nothing of the *Hernia congenita*, and therefore called it a *lusus naturæ* ; it proves that he was informed of all this ; it proves that he came as a friend to see these preparations, and saw them, and heard my brother's opinions and mine upon the subject ; it proves that he knew from my brother's own mouth, that he had made drawings of the parts to illustrate the doctrine ; and Mr. Pott allows, that he never spoke, either to my brother or to me, of his intentions of publishing any thing upon the subject ; yet in a few months after that visit, he published the facts and doctrine, as his own, without mentioning our names in any way whatever : he allows too, that the gentle, but determined rebuke which I gave him, for this singular behaviour, was extorted from me, when a supposed dispute with him was objected to me in reproach : and now after all, and under the weight of these circumstances, he publishes a justification of himself, built upon a *flat* and *positive denial* of these *unquestionable facts* ; and holds it out to the face of the whole world, with an air of triumph. By what name shall we call this species of disorder ?

Οὐκ ἐς' ἀνοίας οὐδὲν (ὡς ἐμοὶ δοκεῖ)
Τολμηρότερον.

If physic has no power, and friends no influence; at least, in such a melancholy situation, means might be taken to prevent all access to such dangerous and destructive weapons as pen and ink.

S E C T. III.

The Circumstances alone sufficient Evidence.

THOUGH, in consequence of what has been laid before the reader in the last section, we may consider the dispute between Mr. P. and me to be at an end, I shall beg leave to shew, that the *circumstances* of the case, without that *positive proof*, would be sufficient to convict him before any impartial tribunal. And in suits of this kind, where positive proof can seldom be had; where no fence can be raised to secure property; where property itself is so dear to the first possessor; where it is so right for the public to encourage invention and improvements, and to discourage, or even to punish plagiarism, it is the duty of all ingenuous men to give judgment from the *circumstances*; to suppose that truth is always attended with an ingenuous, consistent, and open behaviour; and that double-dealing, inconsistency, or contradiction, and misrepresentations of particular parts, are *infallible* marks of an unsound whole. Truth always tallies with, and supports truth; and what is not true, may generally be detected by the nature of the *prop-work* (which must be framed of incongruous stuff) that supports it.

Mr. P. I think, I may say, allows that I explained, in my public lectures, what he called a *lusus naturæ*, before he understood it; for he does not so much as pretend that he knew it before me: he only asserts, that he knew it without me, or found it out himself; and tells us, (p. 143.) this was (without specifying the time or date) when he examined a *Fætus*, in company with an inquisitive young gentleman, at that time his dresser at the hospital, who had injected it, and brought it to his house for examination. This is the fact, which he is pleased to fix upon; and I shall, *for the present*, allow, that it was Lagaranne who put him on the enquiry, with his inquisitive dresser. I must, however, beg leave to ask him, why he did not tell us this inquisitive gentleman's name?

name? or was Mr. Pott afraid it would be found out that he was my pupil? for his dressers and apprentices did me the honour of attending my lectures in those days. Or was he fearful lest I should desire the gentleman to say, upon his honour, whether he had not learned the fact from me, or from my pupils, previously to his meeting, upon an enquiry about it, with Mr. P.? and whether, at that meeting, he had not a better title to be called *communicative*, than *inquisitive*? for I have good reason to believe that Mr. Pott himself was the *inquisitive gentleman*. If I had known his name, I might have asked him likewise, if ever Mr. P. had seen his MS. notes of Dr. Hunter's lectures; and some other questions of that kind. This is a very suspicious setting out. I would ask any man of sense, if he can believe that Mr. Pott, when he was publishing a Treatise on Ruptures, did not ask his dressers and apprentices whether Dr. Hunter had any thing new upon the subject; or desire to see their notes, that he might judge for himself. He allows, that he attended Dr. Hunter's lecture at the theatre, not without hopes of getting some hints upon the subject; and he does not deny, that he desired him to explain his ideas upon a dead body, dissected in private for that purpose. Can any man of sense believe, that his apprentices or dressers did not immediately tell him of a curious discovery, that was made public at a lecture, concerning the subject of his book, and explaining a fact which he had been forced to call a *lusus naturæ*? We see that he talked with those gentlemen upon such subjects. Can he have any reasonable pretext for not being informed of this discovery? Must not information have reached him by twenty different channels? could they all fail? can a man with any *decency*, plead such ignorance? He has not even the plea of distance; the pitiful plea of the Professor, who pretended to have *found out*, at Edinburgh, what at that time was *publicly taught* in the anatomical schools of London, Oxford, Cambridge, and Glasgow.

So far the matter is very clear: now let us trace him down through his own improbable story. "As the thing gave him much pleasure, (p. 144) he procured a number of subjects, examined carefully, noted appearances, drew conclusions, made preparations, and shewed both the papers and the preparations to many of his friends; and, among the rest, to Mr. Serjeant Hawkins, Mr. Sainthill, the late Mr. Nourse, and the late Mr. Webb. When he had examined a great variety of subjects, he enlarged his notes, digested them into better order, and shewed them again to the same gentlemen." But all this time he kept his friend Dr. Hunter in the dark. He consulted his other friends

twice, but him not once. Surely there must have been some good reason for this conduct; because, when he was about writing his book on Ruptures, he took some pains to get a meeting with Dr. Hunter, and was desirous of hearing him explain his ideas on a dead body, procured on purpose. Perhaps he may say, that from the very little satisfaction received at this meeting, he was afterwards less solicitous about having his opinion of any doctrine or observation in anatomy. But, as he tells us, he always was pleased to *entertain a high opinion of Dr. Hunter's anatomical abilities*, it seems strange he should never consult him once, when he consulted his other friends twice. Is not this conduct a demonstration of awkwardness, perplexity, and distress of mind? Accordingly, being sensible of the suspicious appearance, he labours to remove any impression of that kind from the minds of his readers, thus; "Having always," says he, "entertained a high opinion of Dr. Hunter's anatomical abilities, I called at his house, designing to have told him what I had done, and to have had some conversation with him on the subject: The Doctor was not at home, but his brother, Mr. Hunter, was, and with him I had some talk." Here again is the pitiful pretence of a Professor. Both of them would make the world believe, that they had meant to see me; but the Professor called at my house in Jermyn-street, when he knew I was at my lecture-rooms in Covent-Garden; and Mr. Pott called at those rooms in the morning, because he knew I was there only in the evening. He does not pretend that he had made an appointment with me at that time, or desired a meeting with me afterwards, or ever called at my house in Jermyn-street. All this demonstrates that he had no particular desire of seeing me, whatever he may wish to make his reader imagine. What passed between him and my brother, has been related in the preceding section; and he does not pretend that he talked of his papers, or dropped any hint of his intention to publish: Yet he says, in the next page, that his papers were then corrected for the press, and he did not afterwards alter a single syllable in them. Surely, he expressed his own conduct and feelings, when he said of my brother, that *he spake with the most cautious, apprehensive reservedness*. That he did alter some syllables, however, in consequence of that visit to my brother, is clear; because, in the pamphlet, he gave up, or corrected, his error of respiration being the cause of the descent of the *Testis* from the *Abdomen* into the *Scrotum*. The reader must now be perfectly sensible, that this part of Mr. P.'s conduct with regard to me, upon one supposition, is very consistent indeed; but, upon
any

any other supposition, is altogether inconsistent, dark, and absurd; and therefore it must be a strong evidence, with all impartial men, of design, evasion, and under-hand dealing.

Some months after this visit to my brother (which he has so egregiously misrepresented, as was shewn in the second section, and which therefore does not argue, but demonstrate unfair dealing), without seeing or seeking me, he published his pamphlet, and neither named my brother nor me; and this at a time when he knew that all the people of this place, who were conversant with anatomical enquiries, knew that his tract contained nothing material, but what I had made public in my anatomical lectures, before he pretends to have known any thing of the matter; and what I was continuing to make public in succeeding courses of lectures. In this production of Mr. P. the doctrine being transplanted from its native soil, and nursed up in the dark, was imperfect; the descriptions incorrect in some places; no figures of the parts were given for illustration; but three cases from St. Bartholomew's hospital were added, to make up a pamphlet of forty pages; a *time-serving* composition, which was hurried into the world, to snatch the only possible moment for raising reputation; and, if we mistake not, it has raised a reputation which will not easily be shaken off, or soon forgotten.

Almost as soon as Mr. P.'s tract was published, (which he sent me, indeed; for how could he avoid doing so?) I complained of him, by name, in the most open manner, in my lecture; and the Critical Reviewers charged him with plagiarism, when they gave an account of his tract. Yet Mr. P. bore all this without replying, or taking any method of public justification. Had he been conscious of having acted an ingenuous part, it is natural to suppose that he would have justified himself, while dates and other circumstances were recent, and proofs easily procured: for the defence which he has given, at last, is of such a nature as required no great time to be prepared. It contains no testimonies. It is barely the account which he is pleased to give of the matter: his own assertions, without any proof. If his story was true, why did not he, with indignation, answer an accusation made in so public a manner? He knew of it; and says (p. 162) he *restrained* some of the students from speaking of it to me. Why should he *restrain* them from following their inclination, if he knew that his conduct had been proper? Would he persuade the world, that he was afraid it might have hurt my reputation? It is probable, he was afraid it might hurt his own. If I had *spoken to himself*, he says, he would have cleared up the matter;

matter ; but as I had spoken only to about an *hundred gentlemen*, in a lecture, we may presume he thought there could be no reason for taking any notice of it ; and that it could not be supposed to affect his reputation. But why did he take no notice of what was said in the Critical Review ? He tells us, (p. 158) *an anonymous writer has no just claim to an answer* ; and he believes the Reviewers themselves will think so. What, does he really believe that the Reviewers are either so humble, or so abandoned, as to think that no answer is necessary, when they openly charge a man with plagiarism ? Reviewers have character and influence, though they have no name ; and the more influence, indeed, as they profess being impartial : And thence we see authors of this, and of every nation, daily defending their characters, when they think they have been unfairly represented to the public by Reviewers. Why then should Mr. P. trim so nicely, and so patiently, the balance of *just claim*, when his character was so openly attacked ? but, at last, after more than *five years patience*, he found there was a just claim, and published his defence.

After these remarks, it might, perhaps, be thought an affront to the reader's understanding, or candour, to offer farther proofs of something extremely like disingenuity in Mr. P.'s conduct. Yet I will suppose, either that I may have been partial to my own reasoning, or that I may have failed in conveying my ideas clearly. Therefore, I will beg leave to offer one proof more ; which, indeed, is of the most convincing nature : it is this, that the whole story of Lagaranne, which has been so circumstantially related, and upon which Mr. P. rests his defence, by accident has been found to be *an imposition upon the public*.

Some time ago, (about eighteen months, if I can trust my memory) in a conversation upon some points of anatomy and surgery, and particularly upon the *Hernia congenita*, which I happened to have with Mr. Moffat, surgeon to the Middlesex-hospital, and reader of anatomy, he asked me, if I had read De Lagaranne upon *Herniæ* ? Upon my saying I had not, he told me, there was something in that writer, which was very near to a full account of the *Hernia congenita*, and he offered to lend me the book ; adding, that he had shewn it to *Mr. Pott, who was a good deal surprized and pleased with it*. I thanked Mr. Moffat, and told him I had the book, (as well as a thousand more, at least, which, to my shame, I had not read) and that I would certainly look into what the author had advanced.

When

When I observed what use Mr. Pott had made of Lagaranne, in his defence, the misrepresentation diverted me exceedingly. I was very desirous that the public might know the secret; and therefore I wrote a letter to Mr. Moffat, putting him in mind of what he had told me, and complaining of the ill use which Mr. Pott had made of his information. I told him, that he could not be angry with me for telling the truth; nor could he, with honour, refuse to be an evidence in support of it. Then I put some questions to him upon the subject; and he was pleased to send me the following answer.

“SIR,

“If I am called upon, however disagreeable it may be, truth obliges me to declare, that I shewed to Mr. Pott the passage in Lagaranne relative to the processes of the *Peritonæum*, in which the Congenial *Herniæ* are formed. He did not, at that time, seem to be acquainted with the book. I lent it to him, and in a few days he returned it, and told me, that he had long had that book; and intimated, that he had taken notice of the same passage, before I spoke to him; and rather wondered that he had not recollected it. *This was after the publication of his tract*; and, I believe, about the time when that number of the Critical Review was published, which gave an account of his tract. I am, &c.

“Queen-street, Nov. 14, 1763.

J. Moffatt.”

This evidence puts the fiction of *Lagaranne*, and of the *inquisitive gentleman*, in so clear a light, that it requires no comment. What Mr. Pott could say for himself, in this very awkward situation, we shall probably never know; for he has declared that he will write no more upon the subject; and the world may think the declaration was made at a very proper time, *viz.* When his subject was growing intractable and desperate. But my intention being only to convince the candid reader, I will not dwell upon circumstances so humiliating to an author, and to a man.

S E C T. IV.

REFUTATION of absurd ACCUSATIONS.

BESIDES the great points in dispute between Mr. P. and me, which it was *necessary* to settle, some questions have arisen, which would not deserve an answer upon any other occasion; and yet may, with propriety enough, claim some attention, now that the pen is in my hand.

Mr. P. seems to exult in thinking it probable, that I was the author of the account, which was given of his pamphlet, in the Critical Review; and then (p. 159) triumphs over this supposed behaviour, as cowardly and treacherous. In the same page also, he complains, that I attacked him openly at my lectures, and is surprised that I was not ashamed to do it, and ashamed to confess it: *An unmanly method*, says he, *and equally unbecoming a man of candour, or a man of spirit*. Now it seems difficult to conceive, that both those attacks were made by the same hand, they are so unlike: The one was in the dark, and might be treacherous; the other was open, and could only be impudent, if it was at all wrong. It is ridiculous enough to reckon it unbecoming a man of spirit; for, in my mind, an attack made openly, and by name, before a number of gentlemen, and afterwards acknowledged and repeated in print, is not one of the strongest and most decisive marks of the want of a decent share of spirit. I own I should rather suspect the man who, instead of defending himself when he is attacked, stands complaining of the *unmanly* manner, and wrangles about the justice of the *claim*; who disregards one challenge because it has no name, and another because it has.

But, to examine these two inconsistent charges a little more particularly, I must tell the reader, that the account in the Critical Review was not mine, in any other sense than that it was the language I used at the time, both in my lectures, and among my private acquaintance; and therefore the substance of it was, probably enough, delivered by myself to the anonymous person who calls himself *Pupil*, either in a lecture, or in private conversation. I made no secret of the complaint; so that it might easily have been sent to the Reviewers, by any friend of mine. And it is no wonder that two little mistakes should have
crept

crept into the account, without any intention of misrepresenting facts. Accordingly, Mr. P. is there said to have quoted Mr. Cheselden as well as Mr. Sharp, which is an error: but it is an innocent error; for it is not of the least consequence in the dispute; and accordingly Mr. P. who could easily have disproved it, allows it to pass without notice. The other error is this: it is said in that account, that I had complained of Mr. P. *to himself*. This most certainly is a mistake: I never did, and never shall. If I had been the author of the account, that error should not have been introduced, for this reason, among others, that I should not have wished my friends to believe, that I had had any communication with him, after the publication of his pamphlet. From that time he was not to be of my acquaintance; my opinion was totally *changed*; the grounds I went upon were *certain*; and as I was certain that I was ill treated, complaining to him would have been as mean, as it would have been useless.

But, after all, if the account given by the Reviewers, or Pupil, (or, to please him, *by myself*) was *false*, it was unjustifiable, injurious, and infamous: but if it was *true*, as I aver, and have proved it to be, except in the above-mentioned insignificant articles) pray to what purpose is all this wrangling, and accusation about the author of it? Had not I a right to tell first without my name, (if I had thought it proper) what I had before told openly in my public lecture, and afterwards told in a book, to which I put my name? why should not my friends write to the Reviewers? it appears by the Critical Review for June 1757, that Mr. Pott's friends did so.

The other complaint urged against me is, the telling my *tale* to the young people at my lecture-room. "I am really," says he, (p. 159) at a loss to say "which has been most surprizing to me, the Doctor's having made such " complaint, or his not having been ashamed to acknowledge it. Why make " an appeal to a set of people, who could not possibly know any thing of " the matter, or, at least, as it related to me? nor whether the complaint " was well or ill grounded? Why should Dr. Hunter be so vain as to " imagine, that his *ipse dixit* must be implicitly believed by all who heard " him? &c." Has Mr. P. really got into such *habits* and *ways* of thinking, that he is surprized any body *tells the truth*, and is not ashamed to acknowledge it? I have proved every article of that appeal to be *true*: Why then should he be surprized, either that I made it, or that I was not ashamed to acknowledge it? Because, says he, it was made *to a set of people, who could not possibly know any thing of the matter, or, at least, as*

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it related to him. It is very strange, indeed, if the gentlemen who attended my lectures *could not possibly know* what I had *demonstrated there*. Several of them had been present, and bore witness to every part of the transaction; and the rest of them *could not possibly* doubt facts, which were of so glaring a nature, and so well attested by their fellow-students.

Conscious, no doubt, of the absurdity of the first part of his proposition, Mr. P. endeavours to give it a little plausibility by adding, *or, at least, so far as it related to him.* This is another phantom; the mere shadow of an argument. All thinking men must see, that the students could very well know all the material part, even as it related to him. They could read his first book, and then they could not but know, that while I was explaining the *Hernia congenita*, he was calling it a *lusus naturæ*, or accident. Was this above their capacity? They could know from one another (for some were present) that he came, after this, as a friend, and saw the preparations which my brother had made, and which I had shewn to them in lectures, and heard our doctrines and opinions explained. Was this beyond their comprehension? And, as his pamphlet was published when the complaint was made to them, they could read it, and could see that the whole was mine; and yet that he had taken the whole to himself, without mentioning my name, directly or indirectly. Was this dark, or intricate, or beyond their reach? Was it necessary to know more than those facts, to judge of my complaint, or of his behaviour? or, was it necessary, before they could possibly know any thing of the matter, that they should wait patiently five long years, and be made acquainted with the instructive and delectable history of *one Gargantua*, and *the inquisitive gentleman of St. Bartholomew's*? a romance, which it has been already proved, had not an existence, even in the author's fertile imagination, till some time after.

But, says he, (p. 160) "it was disingenuous to endeavour to set me
" in a contemptible light to his hearers, without having once mentioned
" the thing to me, or hearing what I had to say in my own vindica-
" tion." Had his behaviour been only doubtful, I should have endeavoured, some way or other, to have found it out, before I had complained of him in public: but he had saved me that trouble, by removing all possibility of doubt. There was at once an end of our friendship, and of my respect for him. *Fides, ut anima, unde abiit, nunquam redit.* Hear what he had to say! I knew at that time, as well as the reader knows now, that he had nothing to say in his vindication, which could be to the purpose; and yet, it is my sincere opinion, that
he

he lost nothing among his acquaintance, by what he is pleased to call my endeavours to make him appear contemptible. However, I endeavoured to represent him fairly, and as he was: if the figure he made was respectable, the merit was all his own; and if it was not, the demerit was not mine.

It is pleasant enough to see the pains he takes, to make the world believe, that I had been babbling to boys, and mis-leading young minds, who could not judge for themselves. He affects not to know the kind of assembly that he speaks of. There are always a great number of gentlemen present at these lectures, who are enabled, both by education and age, to judge of more difficult questions than any which this dispute has occasioned.

I have now answered all the charges which Mr. P. has brought against me, except what are contained in two notes; and these shall next be considered.

In a note (p. 161) he says, "In the Medical Commentary, speaking
" of my erroneous account of the time of the descent of the *Testes*, and
" of my supposed theft from Haller, the Doctor says, that the subject
" appeared to him *too delicate for conversation*. But though it was too
" delicate for conversation, even with a man whom he dignifies with
" the respectable name of *friend*, yet it did not appear too delicate to be
" made the subject of an anonymous piece of satire. What an idea of
" *delicacy*, as well as of *friendship*, does this convey! *Hic nigræ succus*
" *loliginis; hæc est ærugo mera.*"

That the reader may the better understand the idea that I meant to convey of *delicacy* with my *friend*, and clearly see our author's sophistry, I beg leave to inform him, that in the year 1756 I treated a *very delicate* subject (*viz.* my *friend* Mr. P.'s supposed plagiarism from Haller) *with silence*, because *he was then my friend*: but, after he published his pamphlet in the year 1757, in which he took from me (till then his *friend*) what he knew was mine, and what he knew I should be sorry to lose, without either asking my consent, or making any acknowledgment; then, I say, he had no right to expect *delicacy* or *friendship* from me. Yet, even then, I wrote no *anonymous satire*, but *complained openly* of his most *indelicate* and *unfriendly* behaviour to me. I imagine the reader will now understand the *nigræ succus loliginis*.

—Quod vitium procul afore chartis,
Atque animo prius, ut si quid promittere de me

*Possū aliud, vere promitto. Liberiū si
Dixero quid, si forte jocosus; hoc mihi juris
Cum venia dabis.*

The other note, which I beg leave to answer, is in Mr. P.'s 163d page. It can be a secret to none of Mr. P.'s readers, that he there reproaches me with having *insulted* him, by sending the Medical Commentaries to him, as it contained some things which *could not be pleasing to him*. Very certain I am, that no *insult* was *intended*; nor, indeed, was any civility meant. It was thought but justice, to let the person concerned have a copy of his accusation, as soon as the public; that he might settle the defence he was to make, and be prepared to talk upon a subject, which was to come into public conversation. This, I am told, is always done. I never complained of my antagonists at Edinburgh, for sending me their publications; and never heard, or supposed, that they were offended at my sending mine to them. But, to avoid all unnecessary argumentation, if Mr. P. was really hurt by my ordering the book to be left at his house, as well as at an hundred more in London, I voluntarily give him the satisfaction which a gentleman thinks sufficient in such cases; *viz.* I assure him, upon my honour, that I did not mean it as an offence or insult, and not only beg his pardon, but promise that I will never again send him any book that I may publish.

Thus I have endeavoured to clear up a dispute, which appeared to me to be of consequence. Had the question been only about unimportant discoveries, and insignificant improvements, it could hardly have deserved a line for every page which has been bestowed upon it: But when the characters of men are *staked* in a dispute, it grows too serious and important to be neglected. This consideration made me send these sheets to the press sooner, and perhaps more incorrect, than I could have wished. Mr. P.'s defence of himself, and accusation of me, came upon me in the very beginning of my hurry; in the first week of my first course of lectures, which is not yet finished. If I had had more leisure, I might have put this Supplement into better order, and might have been tempted to touch upon some other inviting subjects. Mr. P. has supplied me with an unnecessary profusion of matter; insomuch that, instead of having *wantonly sought a dispute with him*, as he would have wished the world to believe, I could, for the sake of argument, give up every point that he has defended, and attack him as a plagiarist, upon new ground. I might begin with his anatomical descriptions, particularly

larly with what he says of the rings in the abdominal muscles. He makes a parade upon this subject, as if he was really an anatomical observer and improver, both in his Treatise on the Hydrocele, and in that upon Ruptures; and with as much easy assurance, as if I had not for many years demonstrated the same things, in a very particular manner in my courses of lectures; and as if there were not now living many hundreds of gentlemen, who know the truth of what I here advance. But as I have done some justice to the two principal characters in this dispute, and can have more useful employment for the very few hours that are at my own disposal, I will give Mr. Pott up to the enjoyment of his reputation, as an ingenious and modest improver of surgery, as a man who is faithful to his friend, and religiously observant of Truth, upon every occasion.

Quæ, si singula vos forte non movent, universa certe inter se connexa, atque conjuncta, movere debebunt.

Jermyn Street,
Dec. 31, 1763.

P O S T.

P O S T S C R I P T.

IT would give me a very sincere pleasure, if I could promise myself that I am now appearing in controversy for the last time : I heartily wish that it may be so. I have never attacked any man who treated me fairly, and do promise that I never will. This is a security on my side, that will not fail ; and, we may hope, that the example of my two friends, will prevent the same kind of unfair proceedings from others.

Indeed, my *old master*, Professor Monro, senior, has still a demand upon me ; but he will not permit me to discharge my duty to him, and forces me to take this method of endeavouring to prevail upon him. He has honoured me with an *expostulatory epistle*, and flattered me with the promise of publishing a comment upon all my works. Yet I cannot persuade him, in a more private manner, to answer two short, and plain, and fair questions. Therefore I must lay our correspondence before the public, in hopes that my *old master's* friends will use their influence with him, in my behalf.

He sent his *Expostulatory Epistle* to me, with the following letter :

“ To Doctor William Hunter, Physician, London.

“ SIR,

“ In return for your Commentary, I herewith send you a copy of
 “ some animadversions on the part of it immediately relative to me ; and,
 “ as this is too small a compensation for such an elegant book, I shall do
 “ myself the pleasure to send you a larger volume, of the same kind, on
 “ all your publications, in the vulgar sense, and must, in the mean
 “ time thank you for furnishing such copious materials to

“ Your old master,

“ Edinburgh, Dec. 4, 1762.

Alexander Monro.”

When I had considered his *Expostulatory Epistle*, I wrote to him as follows :

“ To

" To Alexander Monro, senior, Professor of Anatomy, Edinburgh.

" S I R,

" I return you my thanks for the new edition of your Osteology,
" which you were pleased to send me. At the same time I received your
" *Expostulatory Epistle*, and a letter in manuscript.

" You certainly have a right to demand information of the particular
" passages in Dr. Noortwyk's book, which I charge you with having
" misrepresented. They are as follow :

" Medical Eff. vol. ii. p. 119. The words *most strongly* are substituted
" for *quam posset proxime*; which translation alters the sense entirely.

" *Ibid.* The word *and* (*moved the knife*) is substituted for the word
" *vel*; which likewise alters the sense entirely.

" *Ibid.* p. 124. The following sentence is coined: *And the soft*
" *spongy internal substance of the womb is insinuated into the furrows be-*
" *tween these knobs.*

" In my turn, I surely have a right to demand an answer to the two
" following questions, Who is meant by the *deceased benefactor and*
" *friend*? who by the *first introducer into business*, mentioned in the 27th
" page of your Epistle? I flatter myself, you will think it proper to
" give me a direct answer, as soon as your leisure will permit, that it
" may not be in the power of malevolence itself to accuse you of *stab-*
" *bing in the dark.* I am, Sir,

" Your very humble Servant,

" London, Jermyn-street, Feb. 11, 1763. William Hunter."

I expected an answer; and own I was surprized at not receiving any.
At length I wrote to him again as follows :

" To Alexander Monro, senior, &c.

" S I R,

" It is now almost ten months since I troubled you with a letter, to
" which I have hitherto received no answer. I am inclined to do you
" justice; yet I cannot well answer your printed *Expostulatory Epistle*,
" without knowing who are meant by the *deceased benefactor and friend*,
" and the *first introducer into business*, as they are represented by you in
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“ the 27th page of your *Epistle*. Let me repeat to you, that you cer-
 “ tainly had a right to ask, what the passages were in Dr. Noortwyck,
 “ which I affirmed you had misrepresented; and accordingly I pointed
 “ them out to you. I have the same right to be informed of the *bene-*
 “ *factor* and *first introducer*, whom you have endeavoured to make the
 “ world believe I have used ill. Will you, Sir, who (p. 2.) value
 “ yourself upon your *candour*, and (p. 28) recommend plain speaking in
 “ disputes; who call yourself a *blunt, testy old fellow*; will you, I say,
 “ upbraid me, in the face of the whole world, with having behaved ill
 “ to my *deceased friend and benefactor*, and to my *first introducer into*
 “ *business*, and yet refuse to state the fact, in such a way as that I may
 “ clear myself, if innocent: or make the best reparation in my power,
 “ if I have had the misfortune to be so much in the wrong? I cannot
 “ think you will stoop so low; and therefore I will once more ask you
 “ the question, in this private manner, and wait a reasonable time for
 “ your answer. If you will not favour me with an answer at all, you
 “ must not be offended if I apply in another manner, and clear myself
 “ of your *ill-grounded aspersions*. If you were really so informed, you
 “ were egregiously abused, and you will now be glad to clear yourself;
 “ but if you avoid this fair opportunity of doing me justice, I must
 “ accuse you, not only of spreading, but of raising a *groundless calumny*.
 “ I am, Sir,

“ Your humble Servant,

“ London, Dec. 3, 1763.

William Hunter.”

Hitherto the Professor has not condescended to take notice of these letters; and therefore it is now time to address myself to him in print.

To Alexander Monro, senior, &c.

S I R,

Give me leave to send you a *plain letter*, in answer to your *Expostula-*
tory Epistle. Since the publication of that *Epistle*, I have, again and
 again, asked you two plain questions, which your friends will probably
 think, you should have answered sooner. Whether you will now, or not,
 is perfectly indifferent to me; but, for your own sake, it might be pro-
 per to say —; or whatever you have found to go off most speciously
 upon

upon such occasions. You may consult with your relation, whom you have gone some lengths to serve, in his distress:

*Nunc, si quid potes aut tu, aut hic,
Facite, fingite, invenite, effcite.*

And, in the mean time, I will make some short remarks on the rest of your Epistle.

You say, (p. 1) "it was really cruel in me to force you to resume the pen, especially in controversy, which you always disliked so much that you never were the aggressor." This piece of declamation must have entertained your readers, who all know, that in this very dispute, you were the aggressor: and that you wrote a long paper in the Critical Review against me, at a time when I had neither directly nor indirectly brought you into the dispute; and when I had not printed any thing upon the subject. You must allow this fact; and your best friends must allow that it is unanswerable. You will probably best know, what they will say upon this occasion; but I well know what must be their real opinion.

Were you never the aggressor in another instance? Recollect yourself before you speak; and tell the world, who was the author of that coarse attack upon Garengeot, in the Medical Essays, which all gentlemen allow to be a disgrace to the collection.

You tell us (p. 2) that "my late attack in my *Medical Commentaries* on your candour and veracity, the part of your character which you always valued most, piques you so much that you must appeal to the public for redress; and that possibly, when the spirit is thus roused, something more than your vindication will appear." Whatever may afterwards appear, the public, in the mean time, would be glad to see your vindication. Your Epistle is not of that kind: it is vindictive enough, but it is no vindication.

In your 3d, 4th, and 5th pages, indeed, you seem to attempt a vindication; but the attempt ends in nothing. Give me leave to state the case to you. I said that you had forfeited all reputation as an historian, by asserting, first, that you *knew the facts relating to the dispute* (between your son and me) *and sent a fair state of them*; and then by assuring the public, that *Dr. Monro (junior) went to London in absolute ignorance of Dr. Hunter's having any particular opinion concerning Lymphatics, and was surprized when he heard Dr. Hunter teach the doctrine of Lymphatics being*

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Absorbents.

Absorbents. Now, Sir, all the world knows, that it has been proved that this was a *direct misrepresentation* of a fact. Had not I then a right to say so, in my defence? and did not I leave you room, for the only defence which *candour and veracity* could make, *viz.* a confession that your antagonist was in the right, and that you were in the wrong? Would not the public have applauded you more, if you had frankly owned your fault, and pleaded the excuse of ignorance? Instead of this, you wrangle, and will not even confess that it was a fault. Your friends will tell you, that it would have been more proper to deprecate, than to shew a spirit of revenge, in so humiliating a situation.

From the 6th to the 16th page of your Epistle, you wrangle with me about your dispute with Dr. Noortwyk. I have told you the passages which you have *misrepresented*, and the fact is as clear as sun-shine; yet I know that you would wrangle for ever, rather than confess that you have been in the wrong. But there is still one way left you, for gaining a victory over me, in this part of our dispute. State the case to Dr. Noortwyk in a letter; you may have an answer from Holland, in two or three weeks: You allow that he is *learned and candid*; ask him if you have translated those passages like a man of *veracity and candour*, the part of your character upon which you value yourself most: Ask him if he has *altered his opinion*. He is *candid*, you know, and therefore will do you justice readily; and as you allow, that he is *learned*, you will not pretend that he does not understand the meaning of his own words. Your best friends will allow this to be a fair proposal. Try what Dr. Noortwyk will do for a man of *candour and veracity* in great distress.

Unexpectedly, Sir, I am obliged to take my leave of you, very abruptly; but, if I live, this shall not be my final farewell. You shall have the pleasure of hearing from me frequently, till you have gratified my curiosity with respect to my *benefactor and first introducer*. Then, once for all, I will pay my respects to you, and leave you to enjoy the *sweets of your calm retreat*. I intended to have made some remarks upon the rest of your *Epistle*; but while I was writing this *Postscript*, and correcting the proof-sheets of what relates to Mr. Pott, I was so frequently interrupted, that my printer, and many of my friends, began to despair of my finishing what had been promised. At last, on the eleventh of February, I was so fortunate as to meet with a gravid *Uterus*, to which, from that time, all the hours have been dedicated which have been at my own disposal. I have been busy in injecting, dissecting, preserving, and shewing it, and in planning and superintending drawings and plaister casts

casts of it; neither of which can possibly be finished, for some time. You will not then be surprized, that in all this time, I have not once taken up my pen, to finish this *Postscript* on the intended plan. Indeed, it would not have been in my power to have finished it, for some time to come.

I have been so particular in my apology, in order to prevent your thinking me neglectful of you; and likewise that you, who have promised a comment upon all my works, and have thanked me for furnishing such copious materials, may have the pleasure of being informed, that I am preparing more materials for your amusement, and for your criticism. I have already made five very capital drawings from this subject. They, and some more, shall be engraved by the best masters, as soon as possible; and then the whole shall be published. My first and original intention, you know, was to have published ten plates only; but thinking the work imperfect, I waited patiently for more opportunities of adding supplemental figures. Sixteen plates were finished on this plan, several years ago: But still I was dissatisfied with the work, as being incomplete; and, in spite of the importunity of many friends, in spite even of your affectionate and good advice, I kept it from the public. When the additions which have been made, shall be published to the world, I shall have an opportunity of learning whether, for the future, I ought to be directed by your consummate wisdom and prudence, or go on as well as I can, in my own simple and blundering manner. I am, Sir,

Your very humble Servant,

Jermyn-street, March 15, 1764.

William Hunter.

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F I G U R E S.

By WILLIAM HUNTER, M. D.

Physician Extraordinary to the Queen, Professor of Anatomy in the Royal Academy, and
Fellow of the Royal and Antiquarian Societies.

